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A NEW CHAPTER IN

THE STORY OF NATURE.

BY

CHARLES BLAND RADCLIFFE.

7/4603

> "Non trita auctoribus via."
>
> C. Plinii Sec.,
> Nat. Hist., L. i., p. 3.

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PREFACE.

VERY shortly after the publication of the "Vestiges of Creation," at the request of the editor of a periodical of some importance, I wrote a short review of the book which was not allowed to find its way into print because in it I objected to the conclusions of the anonymous author as intrinsically unsound.

I objected to the "Nebular Hypothesis" because the view of heat upon which it is based is inconsistent with that view of heat of which a sketch had just then been made in lines of light by Mr. (now Mr. Justice) Grove in the first edition of his great work on the "Correlation of the Physical Forces." I could not believe that heat in any case was a separate and independent energy: I could only believe that it was a mere mode of some central force of which light and electricity and magnetism and physical force in any other aspect were also modes. I also dimly saw reasons for believing that eventually the source and spring of such a central force would be found to be in an interaction, based on electricity, between the earth on the one hand and the sun and moon and heavenly bodies generally on the other, and that, for want of this interaction, the universe was likely to be cold and dark and forceless generally as well as formless before the day when the earth and the various heavenly bodies were formed and placed in position.

I objected to the doctrine of evolution in organic beings because the grand old law of unity in diversity and of diversity in unity seemed to me to be the law of laws in nature. I looked upon organic beings as invariable when they were left to themselves, and only slightly variable when the conditions of existence were modified naturally or artificially. Except in the way of development up to a certain fixed state of diversity I could not regard anything as rudimentary, and even in any so-called rudiment I could only see that which was perfectly fitted for the organism or organ in that particular state of things. In man I found a *raison d'être* for evolution in the shape of manifold imperfection; elsewhere I could only see a manifest state of perfection in which evolution must necessarily lead to confusion.

I also objected to the doctrine of organic evolution because I saw several reasons for believing that the past history of the earth did not reach back far enough to allow time for evolution to do its work. The wondrous state of preservation in many of the organic remains preserved in the fossiliferous rocks was to my mind a certain proof that natural decay had not had time to do its work before these remains were safely buried out of reach, and in several instances the coal-seams only served to give point to the same conclusion, for certain coal-seams with which I happened to be familiar were so strictly parallel with the rocky strata above and below them, which strata were evidently the product of drifts, as to make it highly probable that the coal-seams themselves, which were as evidently stratified, had been produced in the same way. In point of fact, I found here a cogent reason for disbelieving in the extreme antiquity of the earth, for if the coal-seams were formed of drifts of vegetable matter, of which several may have been formed in a comparatively short time in the same period of submersion, there is no longer any reason to believe that every coal-seam marks a time in which the land had been first upheaved and then submerged, with an interval between upheaval and submersion long enough to allow time for the growth on the spot of the vegetable matter which eventually became coal.

I was then at Paris attending lectures by De Blainville, Etienne St. Hilaire, Brongniart, Serres, Arago, Dumas, and other teachers of great eminence, at the Jardin des Plants, the Observatoire, the Sorbonne, and elsewhere, and around me were several clever friends who were ready enough to talk and to form their own conclusions in this matter, and, as a rule, I think I may say that these conclusions were not adverse to mine. And as it was with me then so it is now, for, notwithstanding all that has been said and done in the forty years intervening between that time and this by Compte, and Darwin and Wallace and Lyell and Huxley and Herbert Spencer, and many others who have followed in their steps, I am still as incapable of accepting the doctrine of evolution as the key to nature, as I was on the day in which I wrote the rejected notice of the "Vestiges of Creation," or in the earlier days in which I became acquainted with what Kant and Lamarck have to say on the same subject.

A glance at the Table of Contents will serve to show what it is proposed to do in the following pages. The doctrine of evolution is dealt with only incidentally: the principal object is to show that a state of electrical interaction is continually going on between the earth on the one hand and the sun and moon and the heavenly bodies generally on the other; that, owing to differences in potential between the earth and moon and between the earth and sun, strong currents are continually passing from the sun and moon to the earth; that the earth is traversed by these currents; that the *heart* of the earth, instead of being, as it is assumed to be, a conductor without resistance, is a conductor with a very high degree of resistance; that the degree of this resistance is inversely proportional to the area of the earth in cross-section at any given point; that at certain points only the currents are frittered down into heat which is fierce enough to reduce rock into the condition of molten lava; that the solar current is so frittered down into heat, not at all points in its track equally, but at two points deep down underground, one on the side of the sun and the other on the opposite side at a point exactly corresponding to it in position; that all that holds good of the solar current holds good also of the lunar current; that these solar and lunar subterranean points of heat are coincident at full moon and new moon, and as far apart as possible at the intermediate lunar quarters; that, as the earth rotates upon her axis, by the movement of these centres of heat, a subterranean ring of fire is formed, under the tropical and sub-tropical regions more especially, which ring thins off, or opens out, towards the poles; that the heat of this ring may change the earth from the form of a sphere into that of an oblate spheroid flattened at the poles, and raise the land above the water; that this change in the form of the earth and this raising of the land above the water is permanent as long as the earth and sun and moon remain in the same position relatively; that an alteration in the direction of the axis of the earth, by doubling and then widening the ring of subterranean fire, may have brought about that mighty revolution by which the ancient land was submerged, and the bed of the ancient sea made to take its place;—and, in addition to all this, that *a tidal wave in the land, which may have an all-important bearing upon the explanation of the oceanic tides, may be a necessary consequence of solar and lunar tidal movements in subterranean heat*, and also that the same action which is concerned in the development of subterranean heat may also be concerned in the development of solar and lunar and stellar heat, and of terrestrial magnetism as well. In doing all this I am brought face to face with the doctrine of evolution again and again, and, as often, I am obliged to see that the past history of the earth can not reach back far enough to allow time for evolution to do any work of importance. Nay, I even find reasons, several of them new, for believing that the earth cannot well be older than 100,000 years, that the reign of ice which intervened between the past state of things and the present need not be of long duration, and that after all the biblical history of the earth and heavens may not be really at variance with the teachings of science.

I cannot hope that what I am about to write, or rather what I have written, for the preface always comes last in the order of production, will be easy and pleasant reading. On the contrary, I am fully persuaded that at present I can only count upon a very few readers, and upon a very scant measure of approval at most. Indeed, in writing what I have written I myself have had all along the feeling that I was dealing with a set *task* which had to be done before I could go home in peace, rather than with a labour of love which carried with it its own reward.

C. B. R.

25, CAVENDISH SQUARE.
2nd June, 1886.

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SUB-CHAPTER I.

ON TIDAL MOVEMENTS IN TERRESTRIAL MAGNETISM WHICH MAY SHOW THAT THE SUN AND MOON ARE DIRECTLY CONCERNED IN THE GENERATION OF THIS MODE OF PHYSICAL FORCE.

IN order to obtain a clear view of the facts with which I have here to do I turn, not to the published reports of any observatory in which magnetic observations are carried on, but to a copy in manuscript of unpublished documents containing the hourly readings of horizontal and vertical magnetic force at the Royal Observatory at Greenwich on the days of spring-tide and neap-tide in 1880—a copy for the possession of which I am indebted to the courtesy of the Ex-Astronomer Royal, Sir George B. Airy. I do not turn to the published reports because these only give certain general means, daily, monthly and annual, of magnetic movements at the hour of noon only. I turn to the unpublished records of magnetic movements at spring-tide and neap-tide, because I can conceive of no better way of arriving at a clearer knowledge of the action of the sun and moon upon magnetic force, which is the end I have now in view, than that which opens out in a comparison of the movements at spring-tide, when the sun and moon, in conjunction or opposition, are acting upon the earth in the same lines, with the movements at neap-tide, when the moon is acting upon the earth at right angles to the sun. And I am, I think, fully justified in thus turning from the published to the unpublished evidence by the results which are set forth in the two following tables. Indeed, I cannot but see that I should have had little or nothing new to say about the action of the sun and moon in causing tidal movements in terrestrial magnetism if I had not so turned.

TABLE I. Means of *Horizontal Magnetic Force* at the Royal Observatory at Greenwich for alternate hours on each of the days of spring-tide and neap-tide in 1880.

	12	14	16	18	20	22	Noon.	2	4	6	8	10
●	·1299	·1299	·1301	·1300	·1293	·1286	·1292	·1297	·1300	·1302	·1304	·1300
☽	·1299	·1298	·1298	·1299	·1295	·1287	·1288	·1295	·1299	·1301	·1301	·1302
○	·1298	·1299	·1299	·1297	·1290	·1286	·1290	·1299	·1300	·1300	·1299	·1298
☾	·1293	·1294	·1294	·1294	·1291	·1282	·1285	·1290	·1296	·1297	·1296	·1294

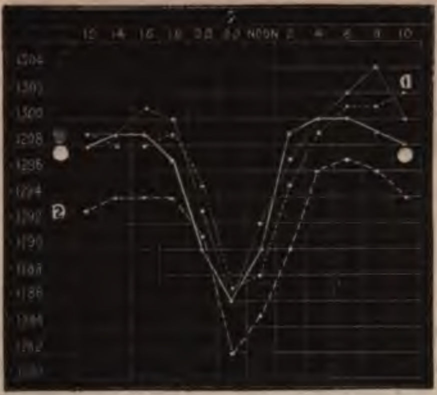
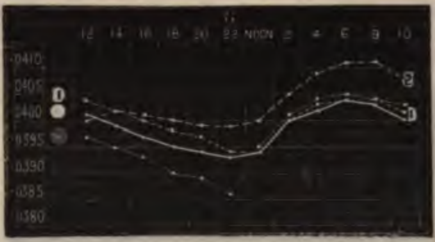


TABLE 2. Means of *Vertical Magnetic Force* at the Royal Observatory at Greenwich for alternate hours on each of the days of spring-tide and neap-tide in 1880.

	12	14	16	18	20	22	Noon.	2	4	6	8	10
●	·0397	·0395	·0393	·0390	·0389	·0386	·0395	·0401	·0404	·0405	·0404	·0403
☽	·0404	·0402	·0400	·0398	·0397	·0394	·0394	·0400	·0403	·0404	·0404	·0402
○	·0401	·0399	·0397	·0395	·0394	·0393	·0394	·0400	·0402	·0404	·0403	·0400
☾	·0400	·0402	·0401	·0400	·0399	·0399	·0400	·0405	·0409	·0411	·0411	·0408



In the facts which are here brought together there is, I think, abundant reason for believing, not only that the movements of the magnetic needles are affected by the sun and moon, but also that the sun and moon have an all important part to play in the development of terrestrial magnetism.

The differences in the hourly readings on each quarter day of the moon, and the differences in the daily readings of the four quarter days, seem to tell their own story very plainly, the former pointing sunwards by marking the progress of the sun from hour to hour, the latter pointing moonwards by marking the progress of the moon from one quarter to another. It can scarcely be otherwise. At all events it may be supposed that the four lines of magnetic force marking the four quarter days, separated by well-marked interspaces, are drawn by the moon and not by the sun—by the moon, because the position of the moon in relation to the earth and sun is widely different on the four quarter days,—and not by the sun, because the position of the sun in relation to the earth is practically the same on each of these four quarter days. Indeed it is easy to see that the four lines of the quarters would run into one line if the sun were alone operative in this matter.

Evidence to the same effect, and more cogent, so far at least as the action of the sun is concerned, may also be found in the variations of terrestrial magnetism exhibited in the tables. For what is the case which has to be dealt with here? It is one in which the magnetism is at a minimum at noon and midnight, and at a maximum in the hours midway between noon and midnight. It is the reverse of what it would be if the magnetism originated in the earth solely, for in that case, instead of varying with the hours of the day the development of magnetism in any place would be the same at all hours. It is exactly as it should be if this origin be in the sun rather than in the earth, for when the point of view as to time is shifted from the earth to the sun, it is easy to see why the development of magnetism at any place on a given hour should not always be the same. The case is indeed one in which the sun must be regarded as having more to do with the development of terrestrial magnetism than the earth—in which it is possible to look upon the sun as winding a magnetic belt loosely around the earth, and as holding it fast, and upon the earth as spinning round within this belt without carrying it along with her.

What then is the meaning of this connexion between the magnetic belt of the earth and the sun? Is the earth traversed by electric currents from the sun? Is the belt of terrestrial magnetism no more than the natural development of magnetism around these currents? Is there a process of electrical interaction continually going on between the sun and the earth, and are these currents to be explained by supposing that the potential of the sun is higher than that of the earth, and that the currents are the natural consequences of these differences in potential? I see several reasons for answering these questions in the affirmative, and none to the contrary. I see that the origin of the currents may easily be accounted for in this way. I see no difficulty in believing that the earth may be traversed by these currents, and that terrestrial magnetism may hold the same relation to these currents which ordinary magnetism holds to any current in any case. I do not see how, in the absence of current, magnetism in its terrestrial or any other form can ever be present. And, therefore, I may fairly consider that in what I have said about the development of terrestrial magnetism there may be a thread of truth which can not easily be torn across.

Nor can it be said that the moon is inoperative in this matter. On the contrary it may be said that the rays of the moon differ from the rays of the sun in being cold, that heat is inimical to the development of magnetism, and that, for this reason, the action of the moon at the neap-tides in, as it were, minimizing this development is less apparent than that of the sun. Indeed, for anything that appears to the contrary, it is quite possible that the earth may be traversed by lunar as well as by solar currents, that each of these currents may have its own magnetic belt, and that, for the reason mentioned, the lunar belt may be masked, for, after what has been said previously, it may be looked upon as certain that the moon is not inoperative in the development of terrestrial magnetism.

And it is also possible that the solar and lunar currents, which are supposed to traverse the earth and to lead to the development of terrestrial magnetism in the way which has been indicated, may be powerful currents. The magnetic needle is made to point north and south by the directive force of the earth's magnetism. The magnetic needle is made to point across a wire or coil of wire along which a current is passed, and if this wire or coil be placed in a line with the magnetic meridian, and the current be strong enough, this pointing is almost due east and west. In reality, however, the current is never strong enough to drive the needle into a position which is exactly at right angles to the direction of the wire or coil, never strong enough to abolish the directive force of the magnetism of the earth. The case is indeed one in which it seems fair to assume that the currents concerned in the development of terrestrial magnetism are more powerful than any current which has been used in the experiment upon the wire or coil, for, so far as is known, the force of the current and the force of the magnetism connected therewith are directly proportionate to each other. Moreover, the high potential of atmospheric electricity at a very short distance above the surface of the earth—a potential which at the lowest has to be measured by several hundreds of volts—may surely be looked upon as a reason for believing that the atmosphere is traversed by very powerful currents. And certainly no contradiction to this notion is to be found in the fact that in 1880 the measurement of terrestrial magnetic force in dyne-units was ·18 for horizontal and ·49 for vertical magnetic force, the practical unit of one ampère being equal to one-tenth of one of these dyne-units.

And thus, as it would seem, it is not too much to say that there are tidal movements in terrestrial magnetism which may show that the sun and moon are directly concerned in the development of this force.

SUB-CHAPTER II.

ON TIDAL MOVEMENTS IN THE TEMPERATURE OF THE AIR AND GROUND WHICH MAY SHOW THAT THE HEAT WHICH IS REGARDED AS WHOLLY SOLAR IS IN PART LUNAR, AND THAT THE SAME ACTION WHICH IS CONCERNED IN THE GENERATION OF TERRESTRIAL MAGNETISM MAY ALSO BE CONCERNED IN THE GENERATION OF SOLAR AND LUNAR AND SUBTERRANEAN AND STELLAR HEAT.

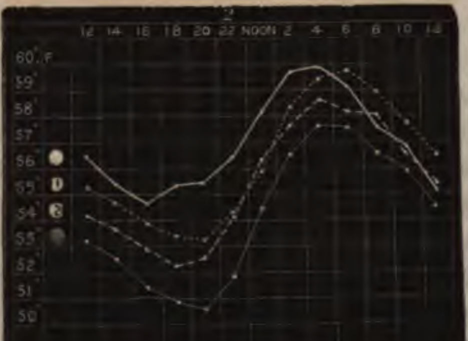
"WHY is the moon so bright and yet so cold?" This question was once put to me in the south of Spain by a shivering courier who felt the cold of the night all the more because the day just passed had been intensely hot, and I shirked the answer by lending wraps, by giving a cigar, and by saying something about the way in which at night, under a clear moonlit or starlit sky, the earth is rapidly cooled by radiation, which something did not satisfy me. It did not satisfy me because I saw that this question might be followed by others equally perplexing. I saw that in passing through space, where for millions upon millions of miles the temperature is as low as −273° F., or absolute zero, the solar and lunar rays must of necessity be cooled down until they have lost every trace of heat, and that it was in reality more easy to say why the lunar rays should remain cold than to say why the solar rays should become hot. I saw that the lunar rays were not altogether heatless, for had they been as cold here as they are far out in space the cooling power of the moon must be at least as evident as the heating power of the sun. I saw that the heating power of the sun was to be regarded rather as something acquired in the lower strata of the atmosphere than as something brought there by solar radiation, and that the same might be said of any heating power belonging to the moon. I saw indeed that the heat which is regarded as wholly solar must be in part lunar. And so it was that instead of mystifying my courier I gave rein to my thoughts in the hope that they would carry me far enough to enable me to find a satisfactory answer, not only to the question why is the moon so bright and yet so cold, but also to several collateral questions.

The hope, thus prompted, has led to enquiries which have ended by showing, as it would seem, very clearly, not only that the heat which is regarded as wholly solar is in part lunar, but also that a common key may be found for solar and lunar and subterranean heat in electrical incandescence—that in fact there is a contention continually going on between electricity in motion and resistance to this movement which may or may not issue in the development of heat, too much resistance interfering with the development of heat by enfeebling the current, too little having the same effect by allowing the current to pass without paying proper toll in the guise of heat. At the commencement of these enquiries I had to go over old ground and to deal with old difficulties. I wanted to find the hourly means of temperature for each of the days of spring-tide and for each of the days of neap-tide, for, as in the case of magnetism, I concluded that any differences in the development of heat which pointed moonwards or sunwards would be brought out most conspicuously on these days. And these means, or the data for framing them, I could not find in any published document. I found daily and monthly and annual means at mid-day only, and I also found that it was the practice to make the readings at no other time. Indeed, I was bewildered for want of information until, in the "Reduction of Greenwich Meteorological Observations from 1846 to 1873," under the tables giving the daily and monthly and annual means of the temperature at noon of the mercurial thermometers showing the temperature of the earth at the depth of 1 inch, 3 feet 2 inches, 6 feet 4 inches, 12 feet 8 inches, and 25 feet 4 inches, and of the air immediately above the ground in the case containing the scales of the "earth thermometers," I found a foot-note in which it is stated that from the beginning of April 1846, when the thermometers were placed in position first of all, to the end of December 1847, when the readings at noon only which have been continued ever since were initiated, these thermometers, with tolerable regularity, were read night and day at intervals of an hour. Without this clue I should have done nothing, for I should have known nothing of the hourly readings in temperature out of which I could get at the particular means of which I stood in need: with it I have been able to do what I wanted, for, by guiding my steps in the right direction, it has led, first, to my becoming possessed of an unabridged copy of the unpublished document containing an account of the observations carried out in 1847 at the Royal

Observatory at Greenwich with the set of thermometers about which I am speaking, and, then, to my using this copy so as to get out of it the means which are set forth in the five following tables.*

TABLE 3. Means of the *temperature of the earth at the depth of* 1 *inch* at the Royal Observatory at Greenwich for alternate hours on each of the days of spring-tide and neap-tide during five months in 1847.

	12	14	16	18	20	22	Noon.	2	4	6	8	10	12
●	53·30	52·56	51·40	50·84	50·64	51·90	54·52	56·64	57·76	57·60	56·66	55·90	54·60
☽	55·30	54·76	53·90	53·44	53·28	54·40	55·94	58·40	59·50	59·74	58·98	57·80	56·50
○	56·50	55·40	54·72	55·36	55·48	56·48	58·16	59·72	59·94	59·10	57·60	56·88	55·10
☾	54·25	53·71	52·91	52·26	52·61	54·16	56·40	57·73	58·71	58·16	58·01	56·56	55·46



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* There ought to be a sixth table, namely that belonging to the thermometer giving the temperature at the depth of 6 feet 4 inches, but, owing to frequent gaps in the record the data for the construction of the table are not forthcoming.

TABLE 4. Means of the *temperature of the earth at the depth of 3·2 feet* at the Royal Observatory at Greenwich for alternate hours on each of the days of spring-tide and neap-tide during five months in 1847.

	12	14	16	18	20	22	Noon.	2	4	6	8	10	12
●	54·07	54·04	54·03	54·02	54·05	54·09	54·16	54·20	54·20	54·17	54·16	54·09	54·09
☽	54·59	54·57	54·55	54·57	54·59	54·59	54·67	54·70	54·74	54·69	54·67	54·61	54·62
○	55·04	55·05	55·03	55·03	55·05	55·11	55·18	55·21	55·21	55·19	55·15	55·07	55·07
☾	53·72	53·71	53·70	53·70	53·71	53·83	53·91	53·94	53·95	53·95	53·93	53·90	53·87

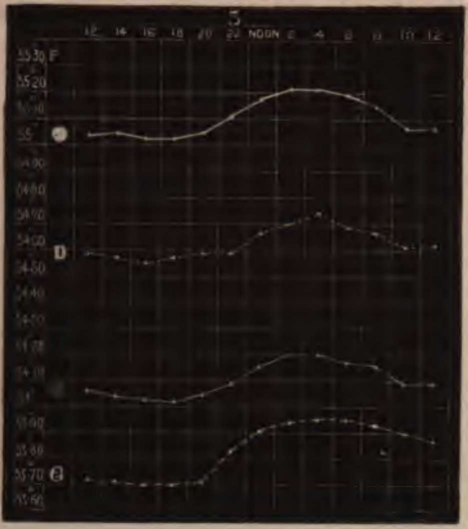


Table 5. Means of the *temperature of the earth at the depth of* 12·8 *feet* at the Royal Observatory at Greenwich for alternate hours on each of the days of spring-tide and neap-tide during five months in 1847.

	12	14	16	18	20	22	Noon.	2	4	6	8	10	12
●	51·18	51·16	51·18	51·18	51·19	51·20	51·22	51·25	51·26	51·25	51·25	51·23	51·23
☽	51·37	51·36	51·36	51·36	51·38	51·33	51·38	51·43	51·42	51·42	51·41	51·41	51·40
○	51·57	51·57	51·56	51·56	51·58	51·59	51·60	51·62	51·64	51·63	51·62	51·60	51·58
☾	50·91	50·91	50·90	50·91	50·89	50·92	50·95	50·96	50·95	50·99	50·98	50·97	50·96

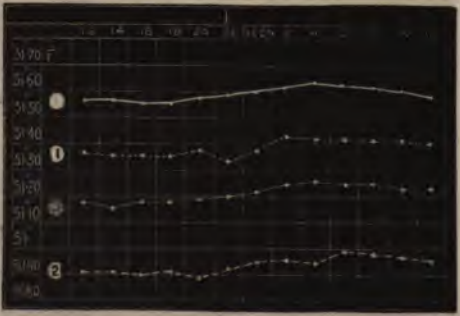


TABLE 6. Means of the *temperature of the earth at the depth of* 25·4 *feet* at the Royal Observatory at Greenwich for alternate hours on each of the days of spring-tide and neap-tide during five months in 1847.

	12	14	16	18	20	22	Noon.	2	4	6	8	10	12
●	49·87	49·88	49·87	49·87	49·87	49·86	49·88	49·89	49·91	49·90	49·90	49·89	49·89
☽	49·97	49·97	49·97	49·97	49·97	49·96	49·97	49·99	49·99	49·99	49·98	49·98	49·97
○	50·02	50·02	50·01	50·01	50·01	50·03	50·03	50·04	50·04	50·04	50·03	50·02	50·01
☾	50·14	50·16	50·14	50·14	50·13	50·15	50·16	50·17	50·15	50·16	50·16	50·15	50·14

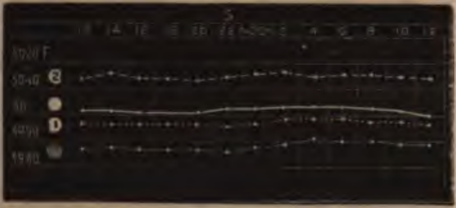
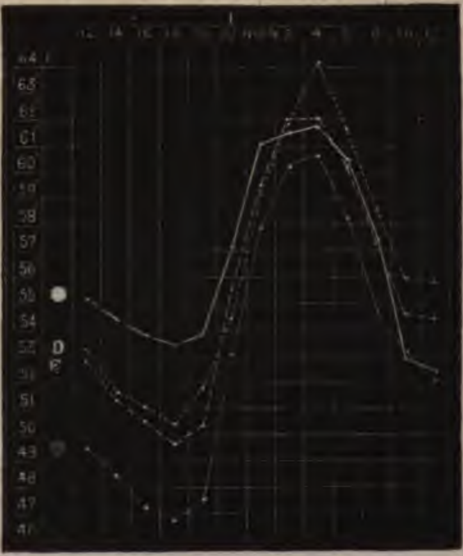


TABLE 7. Means of the *temperature of the air* in the case containing the scales of the "earth-thermometers" at the Royal Observatory at Greenwich for alternate hours on each of the days of spring-tide and neap-tide during five months in 1847.

	12	14	16	18	20	22	Noon	2	4	6	8	10	12
●	49·54	48·48	47·26	46·76	47·60	53·06	57·94	60·25	60·70	58·26	54·70	53·12	52·00
☽	53·30	51·70	51·10	50·40	51·82	54·44	59·14	61·94	64·20	61·70	58·64	55·98	55·82
○	55·26	54·60	53·80	53·46	53·82	57·22	61·06	61·56	61·80	60·46	57·42	52·96	52·36
☾	52·85	51·33	50·50	49·68	50·41	55·50	59·56	62·00	62·05	60·25	57·36	54·66	54·46



After what has been said already it is not necessary to spend much time in poring over these tables in order to see that in addition to the movements which are plainly shown to be solar in their origin by marking the passage of the sun from hour to hour are other movements which are as plainly lunar in their origin by marking the passage of the moon from one quarterly stage to another. Indeed it may be said that there is in these tables that which is altogether unintelligible unless it be supposed that the heat which is regarded as wholly solar is in part lunar.

It is simply impossible to be content with the notion that the rays of the sun are necessarily hot and that the rays of the moon are necessarily cold. It is certain that for millions upon millions of miles out in space the solar and lunar rays alike are as cold as cold can be, for they must partake in the temperature of the path in space through which they travel, which is —273° F., or absolute zero. It can scarcely be otherwise. After having been utterly cold for the greater part of their course the solar rays become hot. After having been utterly cold for the greater part of their course the lunar rays become less cold. The solar and lunar rays alike are altogether heatless in empty space, and more or less heated here. And this being the case, after what has been said about the action of the sun and moon in terrestrial magnetism, it is difficult to avoid the conclusion that the differences in the solar and lunar rays as regards heat may have to be explained by supposing that underlying the rays are electrical currents from the sun and moon to the earth which are frittered down into heat when a certain degree of resistance is opposed to their passage, but not otherwise, the case being not really dissimilar to that which is exemplified in the familiar experiment in which, on passing an electrical current along a chain composed of links of silver and platinum wire placed alternately, the platinum becomes incandescent, and the silver remains cool, or nearly so, simply because the resistance opposed to the passage of the current is high in the former metal and low in the latter.

So regarded it is not difficult to see why as concerns heat the solar and lunar rays should differ as they are found to do. The question is not of rays of heat radiating in all directions from a single source and only becoming cooler as the distance from the source increases—of rays of heat which are always more or less hot: it is one of lines of electrical force connecting two or more points, in which lines heat may or may not be developed by the counteraction of resistance. Between the earth and the sun these lines form a truncated cone with the sun at the base and the earth at the truncated apex, the difference in diameter between the base and the truncated apex being as 900,000 miles to 8,000 miles or thereabouts. The case is one in which, assuming that the current encounters a certain degree of resistance at the base and apex of this cone, and there only, it is supposable that electricity may be frittered down into heat at the earth and not at the sun, for as the resistance is inversely related to the cross sectional area of the cone it may even be that the resistance necessary to the development of heat is present at the earth and absent at the sun. Between the earth and the moon, on the other hand, these lines of force are arranged in a truncated cone with the earth at the base and the moon at the truncated apex, the difference in diameter between the base and the truncated apex being as 8,000 miles to 2,150 miles, or thereabouts. The case here is one in which the resistance opposed to the passage of the current at the base and apex of the cone, and there only, must be less at the earth than it is at the moon, and may be what is necessary to fritter down electricity into heat at the moon, but not at the earth. The case of the earth and moon is indeed exactly the reverse of the case of the earth and the sun, and if heat be the consequence of the contention between current and resistance it is quite conceivable that at the sun the solar rays, which are hot here, may be no hotter than the lunar rays here, and that at the moon the lunar rays, which are cold here, may be as hot as the solar rays in the desert of Sahara at midsummer, or hotter. Nay it is quite conceivable that the solar and lunar rays here may vary in temperature in consequence of some accidental alteration in the resistance opposed to their passage earthwards. Thus, it is possible that the unusual warmth of the two winters before the last may have been owing to the increased resistance to the passage of the solar and lunar rays caused by the presence in the outskirts of the atmosphere of the widespread cloud of dry dust, cosmic or volcanic, to which the brilliant prolongation of the *abendroth* and *morgenroth* was attributable. Thus, it is possible that the clouds may keep the earth warm, not so much by interfering, as a blanket would do, with the radiation of heat from the earth, as by adding to the resistance which is necessary to the development of heat in the solar and lunar rays.

And without over-taxing the resources of imagination or reason, it is also possible to see that the same view may be applied to the explanation of subterranean heat. Indeed all that is necessary to allow of this being done is to suppose that the earth is traversed by powerful electric currents from the sun and moon, and that these currents are frittered down into heat in the way which is exemplified in certain experiments which, with the permission of General Webber, the manager, and with the help of Mr. Mordey, one of the officers, were carried out at the works of the Anglo-American Electric Light Corporation in Lambeth, with the powerful currents used for purposes of electric lighting on a large scale.

Wishing to see how heat is developed in the track of the current across a very bad conductor of which the form is actually or virtually spherical, what was done in these experiments was to pass a powerful electric current across a hollow globe or a shallow circular tray filled with cast-iron filings, diametrically in the former case, from edge to edge through the centre in the latter. The globe and tray were formed of highly varnished papier maché, the globe being a little more than 6 inches in diameter, the tray being about 4½ inches broad by 1 inch in depth. The globe, in addition to having binding-screws on opposite sides for fixing the wires conveying the current, had an open mouth at the top through which the filings and the thermometers, which figure conspicuously in the experiments, could be passed in and out. The temperature was taken at the same time by three mercurial thermometers having scales reaching to 600° F., at three points, B, A, and C, point A being at the centre, point B being on one side of the centre and midway between it and the adjacent electrode, point C being on the other side of the centre in a position exactly corresponding to that of point B. And besides this all that need be said here is that the thermometers were kept in position partly by their bulbs being deeply buried in the filings, and partly by their free ends being attached to the horizontal arm of a suitable stand. The preparations for the experiments with the tray were less complicated, for here no thermometers were required, and all that had to be done was to take the tray, to fit it with binding-screws for the wires conveying the current to the proper places, to fill it with filings, and to pass the current.

The particulars and results which have to be noticed in the experiments in which the globe full of filings was used are those which are here set forth in a tabular form.

EXPERIMENT I.

Direction and Duration of Current.	Thermometer Readings at			Electromotive Force of Dynamo-electric Machine in Volts.
	B.	A.	C.	
+ —→ — 0 min. 30 secs.	200°	190°	300°	800
1 ,,	370°	270°	470°	,,

EXPERIMENT 2.

Direction and Duration of Current.	Thermometer Readings at			Electromotive Force of Dynamo-electric Machine in Volts.
	B.	A.	C.	
+ ← −				
1 minute	105°	68°	120°	700
2 minutes	160°	82°	168°	„
3 „	220°	120°	225°	„
4 „	340°	166°	340°	„
5 „	350°	174°	356°	„
6 „	380°	194°	384°	„
7 „	420°	220°	404°	„
8 „	440°	232°	418°	„
9 „	458°	250°	430°	„
10 „	470°	264°	492°	„

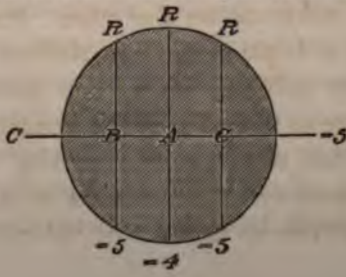
EXPERIMENT 3.

Direction and Duration of Current.	Thermometer Readings at			Electromotive Force of Dynamo-electric Machine in Volts.
	B.	A.	C.	
+ → −				
1 minute	144°	138°	148°	700
2 minutes	180°	140°	181°	„
3 „	228°	148°	236°	„
4 „	275°	160°	284°	„
5 „	320°	180°	320°	„
6 „	350°	196°	350°	„
7 „	378°	218°	376°	„
8 „	398°	234°	396°	„
9 „	450°	250°	410°	„
10 „	476°	268°	430°	„

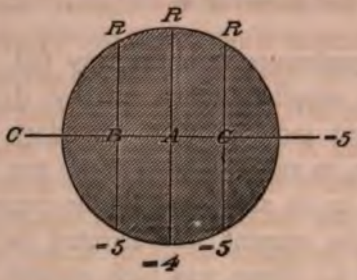
EXPERIMENT 4.

Direction and Duration of Current.	Thermometer Readings at			Electromotive Force of Dynamo-electric Machine in Volts.
	B.	A.	C.	
+ ⟶ —				
1 min.	65°	100°	67°	400
1 „ 30 secs.	83°	145°	80°	„
2 mins.	108°	184°	95°	„
2 „ 30 secs.	137°	220°	110°	„
3 „	150°	238°	120°	„
3 „ 30 secs.	161°	252°	134°	„
4 „	170°	270°	145°	„
4 „ 30 secs.	180°	281°	158°	„
5 „	192°	292°	164°	„
6 „	220°	318°	174°	„
7 „	242°	332°	194°	„
8 „	260°	346°	210°	„
9 „	275°	356°	224°	„
10 „	290°	366°	238°	„

When the more powerful current is used more heat is developed at points B and C than at point A; when the less powerful current is used more heat is developed at point A than at points B and C: and this result, however strange it may seem to be at first, is not to be wondered at. Indeed, it would almost seem to follow as a matter of course if it be allowed, as it must needs be, that in passing diametrically across the spherical mass of iron filings the current must encounter a degree of resistance which, in accordance with the law of Ohm, is inversely proportionate to the cross-sectional area of the mass at any point in its course, and that, in order to bring about the full development of heat, there must be a proper balancing between the action of the current and the counteraction of resistance, too much resistance interfering with the development of heat by enfeebling the current, too little having the same effect by allowing the current to pass without paying full toll in heat. Because it is the same in every part of the circuit, the force of the current, be this what it may, is the same at B and A and C. The force of resistance, which is inversely proportional to the cross-sectional area of the conductor, instead of being the same at B and A and C, at B and C, where it will be the same because the cross-sectional area which is the measure of the resistance is the same at these two points, must be about one-fifth higher than it is at A. The development of heat is at its maximum when the action of the current and the reaction of resistance are duly balanced. Supposing the value of the resistance, R, to be 4 ohms at point A, it will be, speaking roughly, 5 ohms at points B and C. Supposing the value of the current C to be 5 volts, it will be 5 volts equally at the three points B A and C. And therefore the due balancing between current and resistance which leads to the most marked development of heat will be, as is indicated in the accompanying diagram, not at the central point A, but at the two lateral points B and C, the case



being in reality that which is exhibited in the experiments in which the more powerful currents are used. Supposing the value of the resistance to be still 4 ohms at the point A, and 5 ohms at the points B and C; supposing the value of the current to be 4 volts instead of 5; then the most marked development of heat will be, as appears in the accompanying diagram, not at the two lateral points B and C, but at the central point A, the case being in reality that which is exhibited in the experiment in which the less powerful current is used. So that in fact there may be nothing out of order in the seeming irregularities of the experiments under consideration, or in other experiments in which I have found the points B and C shifting until they merged in the point A on the one hand, or until each came in contact with the adjacent electrode on the other. For, after what has been said, it is plain that this shifting towards and merging in the point A of the two points B and C must take place as the value of the current in volts falls from 5 to 4, and also that this shifting towards and coming in contact with the electrodes of the two points B and C must come to pass as the value of the current



in volts rises higher and higher up to a certain point above 5. All this is plain enough if only a moderate degree of attention be paid to the facts of the case. But be the explanation what it may, the facts remain, and it is with the facts and not with the explanation that I am specially concerned now.

In the experiments with the tray full of filings the current was supplied, sometimes by dynamo-electric machines, and sometimes by secondary batteries or accumulators, its value varying from 200 to 400 volts and from 15 to 20 ampères. In the experiments with the globe the current was arrested before the heat developed in the filings was high enough to burst the thermometers: in these, the current was allowed to pass until the filings at certain points became incandescent and molten, and until the parts of the tray underlying these parts were charred or burnt into holes. As in the former experiments so in these it was indeed quite plain that the heat developed in the track of the current was, not at all points in this track equally, but at certain parts only, and also that the position of these points varied with the strength of the current, this position corresponding more or less to the points B and C when the stronger current was used, and to the point A in the contrary case. All this was made plain enough by the incandescent and molten condition of the filings at these points when the current was passing, as well as by the semi-solid or solid nodules of iron, and by the charred or burnt out places in the tray, remaining afterwards.

Before trying these experiments I had been led to the same conclusion by other experiments in which the currents used were supplied sometimes by voltaic batteries, sometimes by dynamo-electric machines, and sometimes by secondary batteries or accumulators. At first, by having greatly underrated the value of the resistance opposed by the filings to the passage of the current, and by having made use of comparatively feeble currents, I failed to get any distinct manifestations of heat. Afterwards, being taught by experience how to act, the thermometers gave feeble indications of heat similar to those which are exhibited so distinctly in the experiments of which an account has been given. The most powerful current of which I made use was in fact not sufficiently powerful to produce any very decided indication of heat except in one instance, where I found that a tray holding filings across which I had passed a very powerful current from several secondary batteries had had two holes burnt in it under two points in which the filings were seen to be incandescent and molten while the current was passing, and were found to be reduced to the condition of solid nodules afterwards. I saw enough, however, to make me not at all surprised at what happened at the works of the Anglo-American Electric Light Corporation in Lambeth. Indeed, I saw enough to justify me in saying that the experiments there are in no way exceptional—that when heat is developed in a body of high resistance of which the form is actually or virtually spherical, the law of development is that which is exhibited in these experiments.

What then? Am I at liberty to believe that there is in these experiments that which is calculated to elucidate the dark problem of subterranean heat? Is the earth, at right angles to her axis, or nearly so, traversed by currents proceeding from the sun and moon? Is the resistance of the earth to be measured by that of the mass of iron filings? Is the solar current frittered down into heat by the resistance opposed to its passage through the earth, not at all points equally in its track, but chiefly at two points, far apart, with the centre of the earth midway between them, and with each of them deep down underground, and equally so, and is the lunar current in the same case? Are the currents under consideration, that is, the solar and lunar currents, comparable in value to the more powerful currents made use of in the experiments? There is, so far as I can see, no reason why it may not be so except one which may be found in the want of resistance in the earth, and at first sight this reason would seem to be fatal. But in fact the actual case is very different to what it is supposed to be. When the circuit of the electric telegraph is completed by a return wire this wire opposes a certain degree of resistance to the transmission of the message. When this circuit is completed by the earth in the ordinary way—this resistance becomes nil or very nearly so. And the law of Ohm shows why it should be so, for if the resistance of a conductor be inversely related to the area of the cross-section, the resistance of the earth, or even of that part of it which is acting as a conductor, must be all but infinitely small in comparison with that of the wire by which the earth is put in communication with the battery of the telegraph. But the earth must be, what Count du Moncel calls *une bonne terre* in order that it may act as a perfect conductor: and, in fact, the evidence supplied by this very excellent observer, who is allowed on all hands to be the chief authority in this matter, is not the only evidence which serves to show very conclusively that the resistance of dry earthy matter is very far from being nil—that the absence of resistance in the earth is very greatly dependent upon the presence of dampness. Usually no doubt the resistance of the earth to the transmission of the telegraphic message may be regarded as practically nothing, but now and then, as Mr. Latimer Clark tells me, the circuit of the telegraph could not be completed by the earth, and it was necessary to use the old return wire before the message would pass, as it was, for example, in the line over the 60 miles of desert between Suez and Cairo.

But the case with which the telegraph engineer has to deal is not that with which I have to deal here. The case with which the telegraph engineer has to deal is that of a current entering into and returning from the earth through channels which are infinitely narrow in comparison with the breadth of the interposed track of earth through which the current has to pass in order to complete the circuit, and therefore it is easy to see that the resistance opposed to the passage of the current in the earth may be infinitely small, even though this part of the earth be no more than a slip of a damp, superficial, shallow shell. The case with which I have to deal is that of solar and lunar currents striking upon *every part* of the hemisphere of the earth exposed to the sun and moon, and forcing their way across the earth by continuing to move on in the same direction. It is one in which there may be no spreading out of currents as in the isolated current of the telegraph, and, for that reason, no reduction of the resistance of the earth to nothing or thereabouts. It is one in which the earth may be, as it were, crowded by currents passing across it, and encountering all the resistance opposed to their passage simply because they are not at liberty to spread out. It is one indeed in which, instead of being nil, the resistance of the earth may be very high, for there is reason to believe that the damp earth which is, as it would seem, chiefly concerned in carrying the telegraphic message does not extend to any great depth below the surface of the earth or below the bed of the sea. Indeed it is not improbable that for thousands of miles the dry and compressed heart of the solid earth may have a very high degree of resistance, for, as is clearly made out by Du Moncel and others, many rocks when dry are very far from being good conductors, marble, in fact, being so very bad a conductor as to occupy, along with cotton wool, paper, and the human body, a place midway between conductors and non-conductors. And thus, after all, it is by no means improbable that the resistance of the earth to the passage of the solar and lunar currents may not differ very widely from that of the mass of iron filings in the experiments under consideration.

And if this be so then it is easy to see that the experiments under consideration may supply a key to the mystery of subterranean heat,—that in fact the earth may be traversed, at right angles to her axis, or nearly so, by electrical currents from the sun and moon,—that these currents may be frittered down into heat by the resistance opposed to their passage across the earth, not equally at all points in their track, but chiefly at two points, far apart, with the centre of the earth midway between them, and with each of them deep down underground and equally so,—that the solar and lunar currents are in the same case in this respect,—that the solar and lunar points of heat are coincident at full moon and new moon, and as far apart as they can be at each of the intermediate lunar quarters,—that the heat so generated is fierce enough to reduce rock to the condition of molten lava,—and that the movement of these heated points as the earth rotates upon her axis may form a ring of fire stretching at least from one tropical line to the other, and thinning off, or opening out, on each side towards the poles.

And if this may be said in explanation of solar and lunar and subterranean heat, it is quite possible that the same explanation may be applied to stellar heat, for between the stars and the earth it is easy to see that there may be the same electrical interaction, the same differences of potential, the same current movement, the same resistance, and the same development of heat if the contention between current and resistance be properly equalized.

Nor is it necessary to change the point of view in order to see why the solar and lunar and stellar rays should be utterly cold in empty space for millions upon millions of miles, and yet more or less hot here. Here these rays are more or less hot because the resistance is present which is necessary to fritter them down into heat: but in *empty* space, for millions upon millions of miles, these rays are as cold as cold can be simply because this resistance is absent. This is all. Nor is heat necessary to account for the extreme tenuity of the material contents of the emptier parts of space, for this state of things may be nothing more than the natural consequence of the operation of unresisted electrical repulsion among the molecules.

And thus it may be right to say that there are tidal movements in the temperature of the air and ground which may show that the heat which is regarded as wholly solar is in part lunar, and that the same action which is concerned in the development of terrestrial magnetism may also be concerned in the development of solar and lunar and subterranean and stellar heat.

SUB-CHAPTER III.

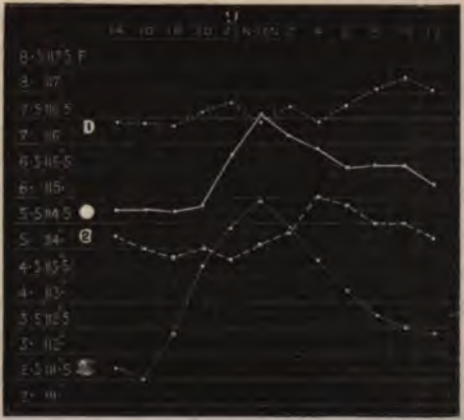
ON TIDAL MOVEMENTS IN THE LAND, ORIGINATING IN TIDAL MOVEMENTS IN SUBTERRANEAN HEAT, WHICH MAY HAVE AN ALL IMPORTANT BEARING UPON THE EXPLANATION OF THE OCEANIC TIDES.

IF, as I have already endeavoured to show, it be true— that the earth is continually traversed by currents from the sun and moon,—that by the resistance opposed to its passage across the earth the solar current is frittered down into heat which may be fierce enough to reduce rock to the condition of molten lava, not at all points equally in its track, but at two points chiefly, far apart, with the centre of the earth midway between them, and with each of them deep down underground, and equally so, and that the lunar current is in the same case,— that these solar and lunar points of heat are coincident at full moon and new moon, and as far apart as possible at the intermediate lunar quarters, — and that as the earth rotates upon her axis, by the movement of these heated centres, a subterranean ring of fire is formed stretching at least from one tropical line to the other, and thinning off, or opening out, towards the poles—it may also be true that every day the heat of this ring of subterranean fire may vary as the earth changes her position relatively to the sun and moon,—that the overlying land may rise and fall in a tidal wave as the temperature of the ring rises and falls,—that this tidal wave in the land may lie in a line which is more or less meridianal,—that any stage in this wave on one side of the earth may be repeated on the other side because, owing to the presence of two (or four) centres of subterranean heat, one (or two) on one side of the earth and one (or two) on the opposite side, any rising or falling in subterranean heat carrying with it a rising or falling of the tidal wave in the land on one side of the earth may be repeated on the other side,—that high land with low water and low land with high water may go together,—and that (if only the heat-producing action of the sun be over-ridden by the heat-producing action of the moon in the same way as that in which the gravitative action of the sun is over-ridden by the gravitative action of the moon), at spring-tide, when, in conjunction or opposition, the sun and moon are on the meridian at or near noon and midnight, there may be high land with low water at or near noon and midnight, and low land with high water at 6 o'clock morning and evening, or thereabouts, and that at neap-tide, when the moon is acting on the earth at right angles to the sun, high land with low water may be at or near 6 o'clock morning and evening, and low land with high water at noon and midnight, or thereabouts. Nor am I writing at random in writing thus, for, as I hope to show before I have done, there is, as I think, good reason for believing, not only that this tidal wave in the land is an actual fact, but also that it has much to do in the production of the oceanic tides.

The results to which I have been led by carrying out certain investigations on the temperature of one of the hot springs at Bath may serve to show, very clearly as I think, that far down below the level to which the heat of the sun can travel by conduction are tidal fluctuations in subterranean heat, which are most differentiated on the days of spring-tide and neap-tide, and which may have something to do in explaining the time of high water at spring-tide in the neighbouring Bristol channel, and elsewhere, if it be supposed that they give rise to a tidal wave in the land. The investigations consisted in getting photographic tracings of the movements of the needle of the galvanometer connected with a pair of thermo-couplets, composed of long portions of stout german silver and iron wire, of which the face was sunk deep down in the hot water of the spring. Without the zealous help of my friends Major Davis and Mr. Alderman Wilkinson, nothing would have been done: and with it, as yet, the only outcome of the work done are the tracings for a couple of lunations, from which, by picking out the days of spring-tide and neap-tide, and by using an arbitrary scale in which two divisions were found to be nearly equivalent to 1° F., I get the data for the means of temperature which in figures and lines are set down in the following table. And this is all I need say here except this—that more is sure to be done presently, partly because the apparatus, automatic and other, which, to begin with, was very defective, is now all it should be, and chiefly because Mr. Wilkinson has been good enough to take matters into his own hands, even to the management of the photography.

TABLE 9. Means of the *temperature of one of the hot springs at Bath* for alternate hours on each of the days of spring-tide and neap-tide during two lunations.

	14	16	18	20	22	Noon	2	4	6	8	10	12
●	2·72	2·50	3·37	4·70	5·42	5·95	5·45	4·80	4·22	3·75	3·50	3·40
☽	7·42	7·40	7·35	7·65	7·80	7·45	7·72	7·45	7·75	8·05	8·20	8·05
○	5·75	5·75	5·72	5·82	6·80	7·60	7·20	6·90	6·55	6·60	6·60	6·25
☾	5·25	5·00	4·87	5·00	4·80	5·10	5·35	6·00	5·85	5·50	5·50	5·25



In this table, and in the tables showing the fluctuations in temperature and in terrestrial magnetism to which attention has been directed already, it may be seen that the development of heat is affected by the movements of the moon as well as by the movements of the sun, hourly differences still pointing to the passage of the sun from hour to hour, quarterly differences still pointing to the passage of the moon from one quarterly stage to another. And over and above this another reason may be found for believing in the existence of an electrical basis for the heat which is here manifested, for such a reason is surely to be found in the fact that in the particular case under consideration the action of the sun and moon, which issues in the development of heat, is one which penetrates very deeply and very quickly into the earth—very deeply, because the variations in temperature in the hot spring at Bath must reach down to a depth below the surface where the heat of the interior of the earth is sufficient to raise the temperature of the water of the hot spring to 117° F., or thereabouts, to a depth, that is to say, which cannot be less than 2 miles below the surface, and which, for that reason, must be very far below the point to which the heat at the surface of the earth can travel by conduction—very quickly, because the hourly and quarterly movements of the sun and moon appear to tell at once in altering the degree of heat. Indeed, in this case, it is difficult to avoid the conclusion that electricity is frittered down into heat, for an action which can penetrate so quickly and so deeply into the earth, and which at the same time can find expression in heat can scarcely be other than electrical in its character. And this too may be noticed—indeed the main reason for calling attention to the facts under consideration is to point to their bearing in this respect—that at spring-tide there is a rise in subterranean heat at noon which may carry with it a tidal wave in the land with high land and low water at noon, and with low land and high water at six o'clock, or thereabouts, and also that on the days of neap-tide there are differences in the development of subterranean heat which may go far to explain why the times of high and low water at neap-tide should differ as they do from the times of high and low water at spring-tide. The evidence upon which I am commenting is, I know, at most only suggestive. Indeed, I should not refer to it if the time of high water at spring-tide in the Bristol Channel not far from Bath was not very nearly what it should be on the supposition that there is at spring-tide that tidal wave in the land, with high land and low water at noon, about which I am speaking.

Nor is it an idle waste of time to talk in this manner. On the contrary the history of the tides from beginning to end may be so read as to point to a tidal wave in the land rather than to a tidal wave in the water.

Evidence to this effect may, I think, be found in the times of high water at spring-tide set down in the Tide Tables authorized by the Lords of the Admiralty. The times of high water at spring-tide here set down are the times at all the places in every part of the world where the times are known, the number of these places being 3020. On the supposition that there is a tidal wave in the water raised by the attraction of the sun and moon, this time everywhere at sea ought to be not very far from noon and midnight, for at spring-tide the sun and moon, in conjunction or opposition, are on the meridian at noon and midnight: actually, when the observations are taken singly, this time would seem to be almost any time, and, when the mean of all the times is taken the result is still more perplexing, for this mean is found to be, not noon and midnight, but 6.15 o'clock. The time, in fact, is as far as it can be from the time when it ought to be on the supposition that a tidal wave is raised in the water by the attraction of the sun and moon. And how is this? Is the mean time of high water at spring-tide at 6.15 o'clock because the tidal wave is in the land, with high land (and low water) at noon and midnight, and with low land (and high water) at 6.15 o'clock? By thus shifting the point of view from the water to the land the case is as it ought to be theoretically, but not otherwise. And thus, without overtaxing the imagination, it is possible to find in the mean time of high water at spring-tide a reason for believing, not only that a tidal wave in the land is an actual fact, but also that this wave may play an all-important part in the production of the oceanic tides.

Nor is this view invalidated by the theory which limits the tidal wave in the water due to the attraction of the sun and moon to the great Southern Ocean, and which looks upon the tidal wave elsewhere as little more than an oscillatory movement set up by the rising and falling of the wave so localized—by the theory, that is to say, upon which the chart of co-tidal lines is based. For when the facts are looked into attentively it is found that the time and the rise of high water at spring-tide at the three great land-ends abutting into this ocean, and at Kerguelen's Land, which island is in the very midst of these southern waters, are these:—

	Time.	Rise.
At Cape Horn...	4.30 ..	9 feet
At Cape of Good Hope ..	9.0 ..	6.6 „
At S.W. Cape, New Zealand..	Noon .	7 „
At Kerguelen's Land.. ..	2.0 ..	2 „

The facts out of which to form a history of the tides in the Great Southern Ocean are these only. There is evidence of a tidal wave in the water which, except in one instance, namely in that of the tide at S. W. Cape, New Zealand, does not keep pace with the movements of the sun and moon. There is little or nothing to make it probable that a tidal wave in the water is raised by the attraction of the sun and moon, and that this attraction tells only on the waters of the great Southern Ocean. And most assuredly the facts do not justify the notion that there is in this ocean that tremendous tidal wave in the water which, with little or no regard to the attraction of the sun and moon, by oscillation simply, or chiefly, is capable of setting up a tidal wave in distant waters, otherwise tideless. In fact, the rise of the tide here is considerably less than the average rise elsewhere. And thus what has been said about the mean time of the tide may be supposed to apply to the time of the tide everywhere.

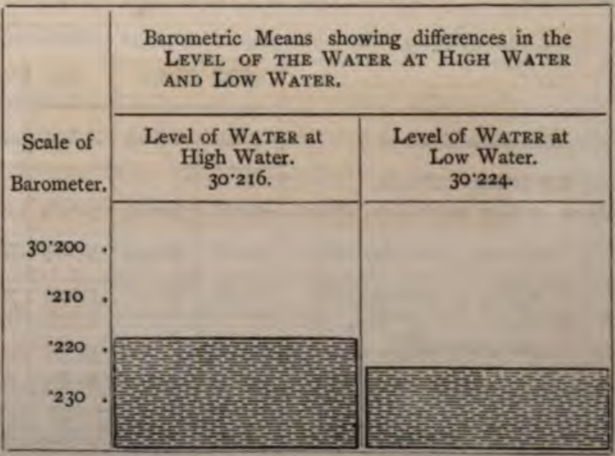
Additional evidence to the same effect may also be found in the means of certain barometric observations which have been carried out for me at the same places afloat and ashore at high water and low water (1) at Waterloo Bridge, London, on the floating quarters of the Thames Police, and on the landing stage alongside, by the superintendent, Mr. Marler, and (2) at Falmouth, on board H.M.S. "Ganges," by the captain of the ship, Captain Bridges, R.N., and at the Meteorological Observatory, by Mr. Kitto, the superintendent—a service for which I shall always remain deeply indebted. At

Falmouth the instruments made use of were the standard mercurial barometer of the observatory for the observations ashore, and the ordinary aneroid of the ship for the observations afloat, neither instrument being removed from the nail on which it hung ordinarily: and here ·130 of an inch had to be taken from the readings of the aneroid in order to bring the two sets of readings into accordance. At Waterloo Bridge the same barometer, a very sensitive aneroid, was used afloat and ashore. As the land or water rose or fell in a tidal wave I saw that the movement must tell in taking from or in adding to the weight of the atmosphere, and, seeing this, I also saw that these barometric observations, when properly checked, would tell me whether the tidal wave was in the land or in the water. The observations were made for these reasons: the results of making them are those which are here tabulated.

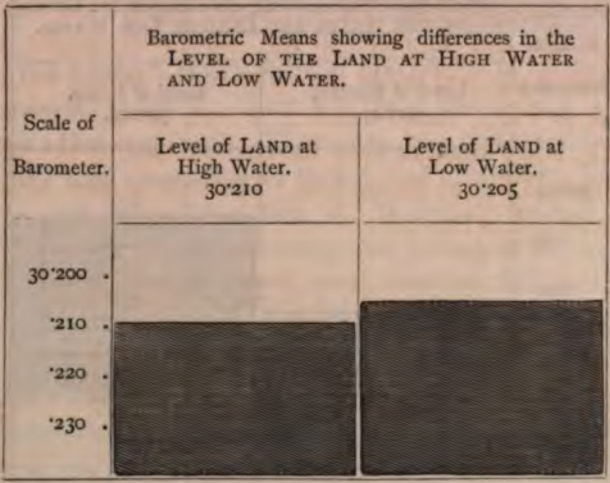
	High Water.		*Low Water.*	
Barometric Means at	*Afloat.*	*Ashore.*	*Afloat.*	*Ashore.*
(1) *Waterloo Bridge.*				
(*a*) From 2 May to 31 May, 1882 ...	30·283	30·284	30·285	30·269
(*b*) „ 24 June „ 21 July, 1883 ...	30·226	30·213	30·215	30·193
(*c*) „ 6 July „ 30 Sep., 1885 ...	30·477	30·478	30·498	30·478
(2) *Falmouth.*				
From 1 Nov. to 30 Nov., 1884 ...	29·878	29·868	29·899	29·883
General Means ...	30·216	30·210	30·224	30·205

The mean differences in barometric pressure afloat and ashore which find expression in these figures are very remarkable.

The mean of barometric pressure *afloat* is less at high water than at low water, being 30·216 in the former case and 30·224 in the latter. The level of the water would seem to have risen and fallen to the extent of 8 feet, for on moving the aneroid up and down the needle was found to rise and fall to a degree equivalent to 0·010 of an inch of the mercurial barometer for every 10 feet of movement. It is not as it ought to be on the supposition that there is a tidal wave in the water only. On this supposition the barometer ought to indicate a rise and fall in the water to the extent, not of 0·008, or 8 feet, but of 0·018, or 18 feet, for at Waterloo Bridge and Falmouth alike the rise and fall of the tide is about 18 feet at spring-tide. The case is that which is exhibited in this table:—

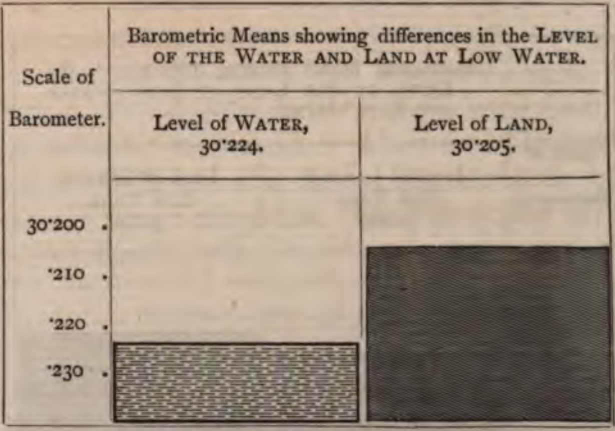


Ashore, on the other hand, the mean of atmospheric pressure is less at low water than at high water, being 30·205 in the former case and 30·210 in the latter. The facts indeed would seem to point, not to the land being at the same level at high water and low water, as it is assumed to be, but to the land having risen at low water and fallen at high water—to, in fact, a tidal wave in the land of which the rise and fall is 5 feet, with high water and low land, and with low water and high land. The case is that which is exhibited in this table :—

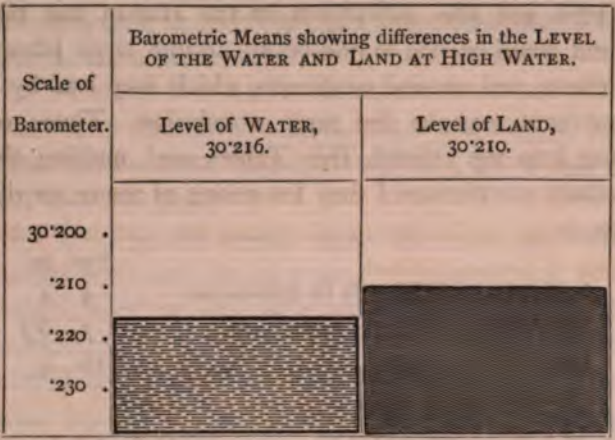


Nor do the barometric means showing the differences in the level of the water and land at *Low Water* and *High Water* tell a different story. On the contrary, they seem to say very plainly that the changes in the level of the land are quite as marked as the changes in the level of the water, and that it is somewhat more easy to believe that low water is caused by the rising of the land, than by the falling of the water.

The barometric means showing the differences in the level of the water and land at *Low Water* are very marked, being 30·224 in the former case, and 30·205 in the latter. The land may have risen or the water may have fallen to the extent of 19 feet, the case being that which is here exhibited.



The barometric means showing the differences in the level of the water and land at *High Water* are less marked than those which belong to low water, being 30·216 in the former case, and 30·210 in the latter. The land may have risen, or the water may have fallen to the extent of 6 feet, as is shown in the following table:



What then? How are these differences in barometric pressure produced? There is a rising and falling in the land: that is evident. There may be a rising and falling in the land amounting to 19 feet, and accounting for the rise and fall of the tide at Waterloo Bridge and Falmouth. There is a rising and falling in the water: that is also evident: but it is not so evident that the rising and falling is tidal, for it is possible that the level of the water would have been the same, or all but the same, at high water and low water, as it was in the first set of observations at Waterloo Bridge, if the result had not been complicated by changes in the weight of the atmosphere brought about by the winds and in other ways in the six hours between high water and low water. And thus after all it may be that the facts under consideration are more consistent with the notion of a tidal wave in the land than with the notion of a tidal wave in the water.

There are also differences in the *rise* of the tide around small islands in mid-ocean, around large islands anywhere, and around continents, which may lead by a roundabout way to the same conclusion. Thus: on diving into the Official Tide Tables and making the necessary calculations I find the means of many entries to be:—

	Feet.	In.
Around small islands in mid-ocean ...	4	4
Around large islands anywhere... ...	7	6·7
Around continents	10	4

On the supposition that the movement of a tidal wave in the water is interfered with by friction against the bed of the sea and in other ways I do not see how these facts are to be accounted for. I can see how the development of subterranean heat may be more marked under land than under water, and that for this reason the rising or falling of the land consequent upon the up and down movements in subterranean heat may be directly proportionate to the extent of the land: for the electrical interaction between the earth and sun on the one hand and between the earth and the moon on the other, to which the development of subterranean heat is traceable, is likely to be more energetic when the land is exposed to the sun and moon, land being comparatively a good conductor, than it is when water is so exposed, water being comparatively a bad conductor. This I can see and only this. And thus perhaps I may be permitted to point to these facts as supplying something in the shape of a reason for believing that the tides have to do with a tidal wave in the land rather than with a tidal wave in the water.

Nor is it difficult to find evidence to the same effect n the exceptionally high and low tides which are met with here and there.

By marking the coasts where the tides are exceptionally high and low on a good geological map of the world it may be seen at a glance that the geological construction of the coast where the rise of the tide is exceptionally high is not the same as that of the coast where this rise is exceptionally low. The coast where the rise is high, so far as is known, is made up of primary rocks, of carboniferous rocks chiefly. The coast where this rise is low, is made up of cretaceous and comparatively modern rocks of various sorts—of rocks, that is to say, belonging to the post-tertiary, tertiary, and later secondary series. Upon such a map, so marked, after what has been said, I find it difficult to look without coming to the conclusion that these exceptionally high and low tides may be due, not to differences in the rise of a tidal wave in the water, but to differences in the rise of a tidal wave in the land, which are themselves dependent upon peculiarities in the expansibility of different sorts of rock under the action of the tidal movements in subterranean heat. This conclusion, as it seems to me, arises not unnaturally out of the premisses, and, so far as I know, there is no valid objection to it. At all events, the current explanation of the tides under consideration cannot be looked upon as satisfactory. There are many bays and channels in which the tide should be as high as it is in the Bay of Fundy and in the Bristol Channel if the unusual rise of the tide in these two instances be really caused by the packing-up, as it were, of a tidal wave in a funnel-shaped channel with its mouth to the ocean. Moreover, the tide is nearly as high as it is in these two well-known instances in several places where there is nothing to interfere with the ordinary play of the oceanic tidal wave, as at Point Darwin and Pierce Point on the N.W. coast of Australia, and at Point Gallagos on the E. coast of Patagonia. Nor is it enough to say that the tides in the Mediterranean and Red Seas are exceptionally low because the doorway between the ocean and the sea is too narrow to allow the oceanic tidal wave to pass in and out freely, for, in fact, tides equally low are met with in places which are as open as they can well be to the ingress and egress of the oceanic tidal wave, as at Cape Palmas and Cape Three Points on the west coast of Africa, at Aulopolai, Negapatam, and Madras on the east and west coasts of Hindoostan, and at Point S. Julian low down on the west coast of South America. In a word the only way of accounting for these exceptionally high and low tides which I can find is one which leads me to believe that I have still to do with a tidal wave in the land rather than with a tidal wave in the water—that some rocks expand and shrink more than others under the action of tidal fluctuations in subterranean heat—and that a day may come in which the rising and falling of the tidal wave may give the key to the geological construction of the sea-bord, when, for example, an exceptionally high tide may be looked upon as pointing, possibly, to the presence of coal at no great distance from the sea in that locality.

It is also possible that certain annual variations in the rise of the tides may have to be interpreted in the same way. At Holyhead, at London, and at Liverpool the level of the tides is considerably higher in December and in January than it is in the summer months. At the mouth of the Swan River in Western Australia the greatest range in any one month very rarely exceeds 3 feet, but the annual range reaches to 5 feet, the highest level being reached in the coldest months of the year. There are also indications of the sea-level being higher in January than in June at Esquimalt in Vancouver's Island, and also in the central parts of the Red Sea, where, as is shown by the exposure of coral reefs, the sea-level appears to be fully two feet lower in the summer than in the winter months. Sir Frederick Evans, R.N., the Hydrographer General of the Royal Navy, who directs attention to these facts, also points out "that the waters of the southern hemisphere attain a higher level at the period of the year when the sun is to the north of the equator, and that the northern waters are highest when the sun is to the south of the line." It is indeed very possible that here also an additional reason may be found for believing that the action of the sun and moon in causing the tides may be upon the land rather than upon the water, for in order to account for the facts under consideration all that is necessary is to suppose that the water is higher in winter than in summer because the development of subterranean heat which raises the land above the waters is less energetic in the winter months of the year than in the summer months. So it may be. And in fact it is more easy to entertain this notion than to believe that these differences in the level of the water are to be explained by the water being attracted towards the sun and moon more at one time than at another, for any conclusion which may be drawn from the state of things in the northern hemisphere is, as it seems to me, flatly contradicted by the contemporaneous state of things in the southern hemisphere, and *v. v.*

And, lastly, it is also possible that the explanation which has been applied to exceptionally high and low tides may have to be applied to the "establishment of the port." If the geological construction of the earth were the same everywhere it may be supposed that the actual time of high water at spring-tide at any place—the time to which the name of "establishment of the port" is given—would be the same at every place on the actual sea-bord. But not so in an earth of which the geological construction is not the same everywhere. For in this case it may be supposed that the actual time of high water at spring-tide will approximate to the mean time of high water at spring-tide, which is 6.15 o'clock morning and evening, when the land rises and falls *quickly* under the action of subterranean heat, and that the contrary state of things will be met with when the land rises and falls *slowly*.

And there is nothing extravagant in the assumption that this tidal wave in the land may vie in magnitude with any tidal wave in the water, for the experiments of Colonel Totten (reported by Lieut. Bartlett in Silliman's Journal for 1852) show that the expansion caused by 1° F. is ·0000579 of an inch in a foot of granite, ·000060816 of an inch in a foot of marble, and ·000114384 of an inch in a foot of red sandstone—is enough, that is to say, to elongate the radius of an earth composed of the least expansible of these rocks to the extent of 102 feet. Indeed the heat at my disposal, even though this be no more than two or three degrees Fahrenheit, is more than I know what to do with, unless I be permitted to believe that some of it is expended in filling up empty gaps of various sorts within the earth.

In watching the tidal dance of the sea upon the shore it is difficult to believe that the reason is here deluded by the senses as it was by the apparent movements of the heavenly bodies before the days of Copernicus. Out at sea, and not very far out, it is less difficult, for there a ship does not swing with the tides as it does at or near the coast. Out at sea, and not very far out, any evidence of any tidal movement is in fact lost. And what is lost at sea, strange to say, is found far inland, namely at Sécheron, a village on the Lake of Geneva. At all events, I may point to certain observations with a spirit level recently carried out at this place for a couple of years by M. Ph. Plantamour at 9 a.m., 12 noon, 3 p.m., 6 p.m., and 9 p.m.,* and say that there are movements of the bubble which show very plainly that in the course of the year the earth rises to a small degree for half the year towards the east, and then falls back to its original level before the year is over, and also that there are every day, towards the east especially, unequivocal risings and fallings of the same sort, the means of the latter movements for each of the days of spring-tide and neap-tide in 1880 being those which are given in this table:—

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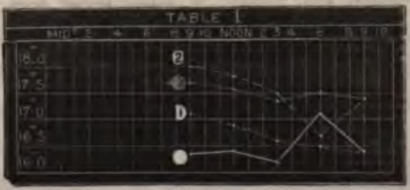
* "Des mouvements périodiques du sol accusés par des niveaux à bulle d'air:" par M. Ph. Plantamour. Archives des sciences physiques et naturelles. Genève, 15 Dec. 1879, and 15 Fev. 1881.

MEANS of certain readings of the movements of the bubble in a spirit-level on the days of spring-tide and neap-tide at the undermentioned hours at Sécheron in 1880.

	9	Noon.	3	6	9
●	17·7	17·6	17·4	17·5	17·4
)	17·1	16·9	16·6	16·5	16·4
○	16·3	16·4	16·2	17·1	16·4
(	18·0	17·8	17·5	16·7	17·4



The story here is only half-told, and all that can be said of it is that so far as it goes it serves to show that the earth is moving up and down when it is supposed to be still, and also to make it somewhat less difficult to believe that the tidal wave in the land, about which I have been speaking, is an actual fact, and that the oceanic tides are brought about by the movement of a tidal wave in the land rather than by the movement of a tidal wave in the water, the land really rising and falling when it seems to be still, the water being really at rest, or moving only so far as to recover its natural level, when it appears to be rising and falling.

And thus it may not be going too far to say that there are tidal movements in the land, originating in tidal movements in subterranean heat, which may have an all-important bearing upon the explanation of the oceanic tides.

SUB-CHAPTER IV.

IN WHICH IT IS SUGGESTED THAT THE WORKINGS OF THE NATURAL CAUSES TO WHICH ATTENTION HAS BEEN DIRECTED MAY GO FAR TO EXPLAIN HOW THE REVOLUTION WAS BROUGHT ABOUT IN WHICH THE BED OF THE ANCIENT SEA WAS CONVERTED INTO THE PRESENT LAND, AND TO ELUCIDATE OTHER DARK PASSAGES IN THE PAST HISTORY OF THE EARTH.

THE action of subterranean heat in causing tidal movements in the land is not, I think, the only action of subterranean heat to which attention may be directed with advantage.

If, as I have endeavoured to show, it be true—that the earth is traversed by electrical currents from the sun and moon,—that the solar current is frittered down into heat by the resistance opposed to its passage through the earth, not at all points equally, but chiefly at two points, far apart, with the centre of the earth midway between them, and with each of them buried deeply, and to the same depth, below the surface, and that the lunar current is in the same case,—that the solar and lunar points of heat are coincident at full moon and new moon, and as far apart as they can be at each of the intermediate lunar quarters,—that the heat so generated is fierce enough to reduce rock to the condition of molten lava,—that the movement of these points of heat, as the earth rotates upon her axis, leads to the formation of a subterranean ring of fire opening out, or rather thinning out, towards each of the poles,—it may also be true—that the heat of this ring of fire may convert a spherical earth, which in the first instance was solid, and cold, and everywhere covered with water, into an oblate spheroid flattened at the poles, with land forced up above the water in certain places, and also that this heat may act in the same way as long as the axis of the earth and the plane of the ecliptic are in the same position relatively, and in a different way in the contrary case. If the heat be sufficient to reduce rock to the condition of molten lava, and if the expansion of different kinds of rock under the action of 1° F. be what it is shown to be in the experiments of Colonel Totten (p. 57), there is without doubt heat enough to modify the form of the earth and to upraise the land in this manner. And, if the premisses are trustworthy, the rest of the statement may be allowed to pass without contradiction, for the ring of subterranean fire, which is the source of the heat, and which remains unmoved in its seat so long as the axis of the earth and the plane of the ecliptic are in the same position relatively, must move in its seat when this position is altered.

After what has been said about subterranean heat the question naturally arises whether the great revolution in which the ancient bed of the sea was converted into the modern land may have been brought about by an alteration in the axis of the earth in relation to the plane of the ecliptic, and, once put, it is difficult not to answer this question affirmatively, and not to perceive that by so answering it the way opens out along which it is not necessary to go far in order to see why the earth was much warmer during the whole of the time spent in the formation of the primary, secondary, and tertiary fossiliferous rocks, and why the present state of things as to climate was ushered in by cold of arctic severity.

There is reason to know that the earth was much warmer during the whole of the time spent in the formation of the primary, secondary, and tertiary fossiliferous rocks, and it will, I think, be well to begin by realizing this fact and by trying to explain it.

"In the greater part of the miocene and preceding eocene epoch the fauna and flora of central Europe were sub-tropical, and a vegetation resembling that now seen in northern Europe extended into the arctic regions as far they have been yet explored, and probably reached the pole itself. In the secondary or mesozoic ages, the predominance of reptile life, and the general character of the fossil types of that great class of vertebrata, indicate a warm climate, and an absence of frost between the 40th parallel of latitude and the pole, a large icthyosaurus having been found in latitude 77° 16′ N. The great development of the tetrabranchiate and dibranchiate cephalopoda, with the general character of the molluscs and corals, as well as of the plants, is in perfect accordance with the inferences deduced from the associated fossil reptiles. If we then carry back our retrospect to the primary or palæozoic ages, we find an assemblage of plants which imply that a warm, humid, and equable climate extended in the carboniferous period uninterruptedly from the 30th parallel of latitude to within a few degrees of the pole, or to northern regions, where at present the severe winter's frost, and the almost universal covering of snow, lasting for many months, preclude the existence of a luxuriant vegetation. A still older flora, the devonian, so far as we have yet traced its geographical distribution, leads to similar inferences, and the invertebrate fauna of the devonian, silurian, and cambrian rocks have such a generic resemblance to that of the carboniferous, permian, and triassic periods as to imply that a similarity of conditions as regards temperature prevailed throughout these six periods."—(Lyell's "Principles," vol. I., p. 231.) Evidence to the same effect is also to be found in the remains, frozen and fossil, of large herbivorous animals (the mammoth, the hairy rhinoceros, the horse, the bison, and others) which abound in the ice-beds and alluvial deposits of very high northern latitudes, for the animals to which these remains belong, or at least those of some of them, could not have existed anywhere near the places where they are buried if they had not been able to procure vegetable food which could not have grown there unless these places were then much, very much, warmer than they now are. In attempting to account for these facts I am driven to side with those who think that the surface of the earth was hotter at this time because the axis of the earth was then perpendicular to the plane of the ecliptic, rather than with those who would explain this state of things by supposing that then more of the internal heat of the earth, the subterranean heat, found its way to the surface because it was not kept back by so thick a crust of solid earth as it is now. The mathematical and other reasons for believing that the earth is solid, as solid as steel, still hold good, and as long as they do so it is impossible to believe that the earth, in accordance with the "nebular theory," is an orb of molten rock of which the surface to a comparatively trifling depth has become solidified by cooling.

And this is not the only evidence against this theory which may be cited, for after what has been said about heat, it is scarcely too much to say that the earth and the universe generally are most likely to have been as cold as cold can be before the day when the various heavenly bodies, as *bodies*, began to interact electrically. And, therefore, in trying to explain why the earth was warmer during the whole of the time spent in the formation of the fossiliferous rocks, the only course open to me, of which I know anything, is to side with those who believe that then the axis of the earth was perpendicular to the plane of the ecliptic, and that thus, by being in a state of perpetual equinox, the earth was warmer everywhere by being winterless everywhere. This course I may adopt without doing violence to any sound rules of reasoning: and, unless I deceive myself, this course I must adopt if I would push the inquiry in which I am engaged to a successful issue. At all events, rightly or wrongly, what I now do is to assume as a starting point that the axis of the earth was perpendicular to the plane of the ecliptic during the time spent in the formation of the primary, secondary, and tertiary fossiliferous rocks.

In seeking to explain how it came to pass that the axis of the earth was moved into its present position in relation to the plane of the ecliptic there is no occasion to call in the help of miracle, for in order to bring about this result all that may be necessary is to suppose that the earth fell foul of an aërolite of a certain size in a certain way. And this supposition cannot be set aside as simply frivolous, for it is possible to believe that this very aërolite was shivered into fragments by this collision,—that these fragments still go on circulating in the same orbit as that in which the parent aërolite moved,—that the angular meteorlites which now and then fall upon the earth are some of these very fragments,—and that another tilting from the same cause is within the range of possibilities.

Nor is it difficult to see that this tilting of the axis of the earth may have led to that mighty revolution in which the bed of the ancient sea was converted into the present land.

If it be allowed, as it surely may be, that the development of the subterranean ring of fire is more marked under the parts of the earth on which the rays of the sun and moon fall directly than it is under the parts on which they fall obliquely, and that, owing to its great depth below the surface, the ring, once formed, must be very slow in losing heat by conduction, it is easy to see that this ring would be altered in more ways than one by the tilting of which I have now to speak. Before the tilting, the sun and moon being then directly over the equatorial region at all times, the ring, and the land upraised by its heat, may have occupied a position which did not extend far north and south of the line: after the tilting, owing to the fact that in the course of the year every part of the inter-tropical region would be directly exposed to the direct action of the sun and moon, the ring, and the land raised by its heat, must widen very considerably. If the position of the earth immediately after the tilting was that occupied at one of the solstices, there may then have been two rings of subterranean fire, neither of them very broad, one under the equatorial line, the other under the solstitial line. Subsequently, as the year rolled on, other rings may have formed so as to fill up, first, the space between the solstitial line and the equator, and, afterwards, the space between the equator and the other solstitial line, in which case the old ring, and the overlying land upraised by its heat, may have gradually widened until it took in all the inter-tropical region, and, not improbably, a wide extent of the outskirts of this region as well. In this way, at the time of the tilting, a new ring of subterranean fire may have been formed upwards of fifteen hundred miles away from the old equatorial ring, and under a part of the earth which was then covered with water. In this way, by the heat of this new ring and of other new rings following it as the days wore on, the bed of the ancient sea may have been raised until it became the present land. In this way, by the rising of its bed up to a certain point, the ancient sea may have been forced over the ancient land until the state of things was no other than that of a universal deluge. In this way, by the rising of its bed beyond this point, the sea, seeking its own level, may have settled down into its present bed, and so left the land dry as it is now. All this appears to follow almost as a matter of course Indeed it is not necessary to make any violent assumption in order to see how the great revolution in which the bed of the ancient sea was converted into the present land may have been brought about by the shifting of the axis of the earth from a position perpendicular to the plane of the ecliptic into its present position.

And, this being the case, it is not to be wondered at that the present state of things as to climate should have been ushered in by a time of terrible cold, for the state of universal deluge of which I have been speaking would carry with it a state of refrigeration in which the ice-caps of the poles would be drawn together until their edges were upon the point of meeting at the line.

The earth is heated chiefly by an action of the sun which tells more upon the land than upon the water: and the climates of the earth would be much altered by any marked difference in the distribution of land and sea. With land in the tropics and water elsewhere the earth would be much hotter than at present: with this state of things reversed, the earth would be much colder than at present, the latter arrangement, as Lyell pointed out long ago, being likely to produce a glacial epoch in regions now temperate and subtropical. And if so, it surely follows that colder still must have been the earth when covered everywhere with water, and that I was using no mere figure of speech when I said that a state of universal deluge would carry with it a state of refrigeration in which the ice-caps of the poles would be drawn together until their edges were on the point of meeting at the line.

Nor is unusual keenness of vision necessary in order to see that the new land, which is the present land, would lie under enormous glaciers until the ice had time to melt away, and that the action of these glaciers may have to be taken into consideration in trying to account for the reign of frost by which the present state of things as to climate was ushered in.

The facts pointing to glacial action on a tremendous scale as having been at work in post-tertiary geological times are unmistakable.

The climate of this country, and of a very great part of northern Europe and America, was as severe as that of Greenland and Spitzbergen in the time intervening between the formation of the fossiliferous rocks and the advent of the present state of things. Everywhere in these regions are signs which show that the present land was then buried under enormous glaciers. Rocks in all directions are grooved, and scored, and scratched, and rounded off, and planed down, and polished, exactly as they now are by glaciers still at work, or which were at work not long ago. Fragments of rock, evidently brought from a distance, and often from a great distance, are found heaped together in a way which is only intelligible on the supposition that they are actually the moraines of some ancient glacier. Erratic blocks of rock, as evidently brought from a distance, and often from a great distance, lie scattered about on the ground, or half buried in the ground, as *roches moutonnées*, or perched up on pinnacles of various sorts, in a way which is unintelligible unless it be supposed that at one time they lay upon, and were carried along by, some ancient glacier. And the same story is told by the sort of deposit to which the name of "till" is given, and by the planed and scored rocky surface often met with under the "till." For "till" (which is an unstratified deposit of tough clay or half hardened stone in which are embedded rocky fragments of various sizes and shapes, some of them smoothed down and polished and scored, and others not so) is evidently nothing more than an inspissated and hardened form of the sticky and unctuous mud, mixed with rocky fragments of various shapes and sizes, marked or not marked by ice-action, which is now met with under a glacier, and which is formed by the grinding of the glacier against its bed—by that very grinding to which the planed and scored surface of this bed also bears witness often enough. In fact, it cannot be doubted that the sub-tropical period in which the fossiliferous rocks were deposited was followed for a time by one or more glacial epochs, and the only question is whether this great fall of temperature was continuous so as to form one glacial epoch only, or interrupted by interglacial periods of warmth so as to form more than one such epoch. The occasional presence between layers of "till" of deposits containing remains of plants and animals which could have only flourished in a warm climate is supposed to point to these warm interglacial periods, but it is quite possible that these remains may have been brought to the places where they are found by one and the same glacier having happened to plough its way through fossiliferous strata which may have been formed when the axis of the earth was perpendicular to the plane of the ecliptic, and when the earth was warmer by being perpetually in a state of winterless equinox. And there is no difficulty in entertaining this notion, for it is highly probable that, in addition to the other signs of ice-action, many deep valleys and many extensive denudations in the tertiary strata and chalk have been formed at the same time by the scooping-out and planing-down action of some tremendous glacier, and that in this way the fossils which are supposed to point to warm interglacial periods may really point to tertiary and secondary fossiliferous rocks which were formed in the warm times which preceded the present state of things as to climate, and which were broken down by, and carried along under or on the glaciers which covered the greater part of the modern land until the ice had time to melt away. Indeed, there are reasons, with which I shall have to concern myself presently, which make me more ready to entertain this notion than to believe that there have been many glacial epochs separated by warm interglacial epochs, the glacial and the interglacial epochs alike extending over periods of time well-nigh interminable. At all events there is some show of reason for believing that the revolution in which the bed of the ancient sea was converted into the present land was attended by a time of terrible cold,—that the new land was covered with tremendous glaciers until the ice had time to melt away,—and that the deposits among the signs of ice action containing the remains of plants and animals which can only have flourished in a warm climate may point rather to the ploughing of one and the same glacier through fossiliferous rocks which had been formed long before than to warm interglacial epochs which have to be accounted for by slowly recurring astronomical causes.

And thus it is quite possible that the workings of the natural causes to which attention has been directed may go far to explain how the revolution was brought about in which the bed of the ancient sea was converted into the modern land, and to elucidate other dark passages in the past history of the earth.

SUB-CHAPTER V.

IN WHICH IT IS SUGGESTED THAT THE HISTORY OF THE EARTH MAY NOT REACH VERY FAR BACK INTO IMMEMORIAL TIME.

IT may be looked upon as certain that in regions now temperate and sub-arctic the climate was sub-tropical or tropical while the primary, secondary, and tertiary fossiliferous rocks were in course of formation (p. 65). It may be looked upon as certain that the present state of things as to climate was ushered in by one or more glacial epochs of arctic severity (p. 71). It may be looked upon as certain that a long time was spent in the compilation of the "records of the rocks" from which these conclusions are drawn: but it cannot be looked upon as certain that interminable ages are comprised in the time so spent. Indeed the more I look into the matter the more firmly am I convinced that this time may be shortened until it is brought within comparatively narrow limits.

Among the reasons for thinking that interminable ages have not been spent in the formation of the fossiliferous rocks is one which to my mind is almost conclusive, and to which, strange to say, attention has not been directed, and this is to be found in the absence of any signs of glacial epochs among these rocks. For, as will be seen in the following table, in which Sir C. Lyell places side by side the results of the laborious calculations of Dr. James Croll and Mr. John Carrick Moore, several glacial epochs must have left their marks among the fossiliferous rocks if the time spent in the deposition of these rocks reached no further back than a single million of years.

Table showing the variations in the eccentricity of the earth's orbit for a million years prior to A.D. 1800, *&c.*

	1.	2.	3.	4.
	Number of years before A.D. 1800.	Eccentricity of orbit.	Difference of distance in millions of miles.	Number of winter days in excess.
	0	·0168	3	8·1
	50,000	·0131	2¼	6·3
A	100,000	·0473	8½	23·0
	150,000	·0332	6	16·1
B	200,000	·0567	10¼	27·7
	250,000	·0258	4½	12·5
	300,000	·0424	7¾	20·6
	350,000	·0195	3½	9·5
	400,000	·0170	3	8·2
	450,000	·0308	5½	15·0
	500,000	·0388	7	18·8
	550,000	·0166	3	8·0
	600,000	·0417	7½	20·3
	650,000	·0226	4	11·0
	700,000	·0220	4	10·2
C	750,000	·0575	10½	27·8
	800,000	·0132	2¼	6·4
D	850,000	·0747	13½	36·4
	900,000	·0102	1¼	4·9
E	950,000	·0517	9¼	25·1
	1,000,000	·0151	2¾	7·3

Explanation of the Table.

Column 1. Division of the million years preceding A.D. 1800 in 20 equal parts.

Column 2. (computed by Dr. James Croll by aid of Leverrier's formula) giving the eccentricity of the earth's orbit in parts of a unit equal to the mean distance or half the longer diameter of the ellipse.

Column 3 (computed by Mr. John Carrick Moore) giving in millions of miles the difference between the greatest and least distances of the earth from the sun during the eccentricities given in column 2.

Column 4 (also computed by Mr. Moore) giving the number of days by which winter occurring in aphelion is longer than the summer occurring in perihelion at these different epochs.

Here then are facts which show that in the last million years the orbit of the earth must have undergone great variations in eccentricity in the five periods to which the letters A, B, C, D, and E are prefixed. At present the eccentricity of the orbit amounts to three million miles: in these five periods this eccentricity was 3, 3·5, 3·5, 4·5, and more than three times greater than it is now. Now the winter here is in perihelion and the cold very moderate: in these five periods, with winter at the aphelion, the cold here would be as intense as it is in Greenland or Spitzbergen, for then, when most in aphelion, the earth would be nearly 11 million miles more distant from the sun, and the winter 36 days longer, and the heat of the sun one-fifth less intense, the summer, even though much hotter in consequence of the earth being in perihelion, not serving to melt all the snow and ice which had fallen and formed in the winter. These periods of great cold must have continued for a long time, and have recurred oftener than the five times which are indicated in the table. They must have continued during a very considerable portion of the time occupied in each precession of the equinoxes, which amounts to 21,000 years. They must have continued during a very considerable portion of the time occupied in each revolution of the apsides, which portion would be long enough to include many precessions of the equinoxes. There must, in fact, have been several times which were no other than glacial epochs—times long enough for the flora and fauna to acquire an arctic complexion, and for beds of "till" and other signs of glacial action to be formed. I do not see how it could have been otherwise. And if so, then the only conclusion which can be drawn from the absence of any signs of these epochs among the fossiliferous rocks is that the time spent in the formation of these rocks fell far short of a million years—that in fact it did not reach back as far as one hundred thousand years, when, as is seen in the table, the orbit of the earth would for the first time exhibit a high degree of eccentricity.

Nor do I lose count of time when I attempt to ascertain the age of the carboniferous and calcareous strata.

Very possibly many ages may have been spent in the formation of the coal-seams. But to speak of many ages is not quite the same thing as to speak of numberless ages. The forests which supplied the material for the coal-seams grew in times when the climate of the place was tropical or sub-tropical. They were composed chiefly of flowerless or cryptogamous plants and trees, of the families of fern, club-mosses, and horsetails, of which the growth may have to be measured by that of fungi rather than by that of shrubs and trees of a higher grade; and in fact I can find no conclusive reason for supposing that very many thousands of years should have been spent in this growth. It is also very possible that many coal-seams may be made up, not of vegetable material which has lived and died on the spot, but of vegetable material which has *drifted* there. On this supposition it is easy to account for the absence of roots and soil on the under surface of many coal-seams. On this supposition it is easy to see why the coal-seams, like ordinary sedimentary rocks, should, as they do, split in a plane which is or once was horizontal. On this supposition it is easy to see why the coal-seams should be, as they are, parallel to the other strata above and below them. On this supposition it is easy to see why the coal-seams may, as they often do, attain a degree of thickness which is altogether inexplicable if the materials entering into their formation grew on the spot—grew, that is to say, long after the soil must have been utterly exhausted. Nor is it necessary to turn elsewhere in order to account for the "fossil forests" which are continually cropping up on the shores of the Bay of Fundy as the softer material in which they are imbedded is carried away by the wash of the sea. For what is the actual case in respect of these trees? It is that some of them have been uprooted and laid prostrate; it is that others, although erect, are without roots; it is that many, with roots pointing downwards, are rooted in rock which may have been sludge, but which could never have been soil. The case, in fact, is more explicable on the supposition that these fossil trees are not trees which have lived and died on the spot, but ancient "snags," which, like the modern "snags" of the Mississippi, have drifted from a distance to the spot where they are now buried.

Again, drifts may have had an important part to play in the formation of many calcareous strata. In the oolite the corals are often laid flat like the shells with which they are mixed, and in the marlstone bands met with in the lias of Yorkshire the oysters are chiefly single valves, many of them water-worn, and all of them looking as if, like the corals and shells in the oolite, they had drifted into their present position. There is indeed nothing like a modern coral reef or a modern oyster bed in the strata which are almost entirely made up of the fossil remains of the creatures which once lived on such reefs and beds. And these cases are by no means exceptional. Nay, it is even possible that a good deal of the coral rag and white chalk and flint deposits now met with in the crust of the earth might have been formed in the course of a few thousand years if the coral polypes and microscopic globigerinæ and other foraminifera, and the microscopic radiolariæ and diatoms, worked as hard in ancient times as they do now. Moreover, the work of forming chalk or flint may have been in great measure a simply chemical work, the material being deposited, perhaps rapidly, from water in which it had been dissolved previously, just as travertine, which is one of the forms in which limestone presents itself, is deposited quickly when the water in which it was dissolved by the help of free carbonic acid loses a certain portion of this acid.

To my mind also the mere presence of any fossil in which the petrification extends to soft and perishable textures is a fact which points very plainly to the speedy formation of the rock in which the fossil is imbedded. Only the rapid accumulation of the inorganic imbedding material could have preserved these textures from decay and dissolution. And this idea is emphasized by the distorted and shattered condition of the fossil remains of the icthyosauri and other large reptiles in the trias, for this state of things is scarcely to be accounted for except on the supposition that these animals, alive or dead, were crushed under the thick and heavy coverings suddenly heaped upon the bed on which they happened to lie. In point of fact, by so regarding them, the fossils themselves in many instances supply abundant reason for believing that long ages were not spent in the formation of the fossiliferous strata.

Nor is the actual arrangement of the fossils in the rocks that which compels me to believe that while the rocks have been forming a process of evolution has been going on in plants and animals which could only be accomplished in countless ages.

The order in which the fossils are ranged in the several strata is very far from being in full accordance with the doctrine of evolution. In the earlier primary rocks (the lower and upper silurian) are certain cryptogams (algæ and lycopodiaceæ): in the later primary rocks (the devonian, carboniferous, and permian) are these and other cryptogams, together with very many conifers (which are flowering plants with naked seeds) and a single monocotyledon, the *pothocites grantonii.* In the secondary rocks (the trias and lias, the oolitic and cretaceous groups) cryptogams in still greater number are met with, with conifers and cycads (the other family of flowering plants with naked seeds) in abundance, and several monocotyledons. In the tertiary and post-tertiary rocks (eocene, miocene, pliocene, post-pliocene, and recent) cryptogams, gymnosperms, monocotyledons and dicotyledons, all the leading forms of vegetable life now existing, are fully represented. In all the fossiliferous rocks are examples of all, or almost all, the invertebrata, of the highest types as well as of the lowest, all mixed together in a way which makes it impossible to say that the simpler forms are first in order. Some forms, like the crinoids, are more abundant in the primary than in the secondary and tertiary rocks; others, as the trilobites, are present in the primary and absent in the secondary and tertiary rocks; but the plain fact is evidently this—that examples of all, or nearly all, the invertebrata, mixed together almost indiscriminately, are met with in all the fossiliferous rocks. Nor is the case altogether different with the vertebrata. In the upper silurian, the devonian, the carboniferous, and the permian rocks are plagiostome fishes (sharks and rays); in the two last named rocks, together with these fishes, are many ganoids, many labyrinthodont amphibiæ, and a few lacertilian reptiles, but no birds and no mammals. In the secondary rocks fishes are more abundant, reptiles, many of them of great size, and birds and marsupials are present, several marsupials being met with in the trias, or lowermost secondary stratum. The oldest fish (pteropsis) which is met with in the upper silurian rock, is by no means of the lowest grade; and Professor Owen, speaking of fossil fishes generally, says that the idea imparted by their history is that "of mutation rather than that of progression." And lastly, in the tertiary and post-tertiary rocks are examples, not only of fishes, and reptiles, and birds in plenty, but also of almost all the mammals now existing, all mixed together as indiscriminately as are the invertebrata. It is indeed very difficult to find conclusive evidence in favour of evolution in these facts. The fossils of the tertiary and post-tertiary rocks plainly show that the plants and animals then existing were divided into the same great groups as those now met with, and the natural inference appears to be that in ancient times as now there was a system of things in which a vast crowd of dissimilar plants and animals had to minister to a common purpose, each organism having a part to play which was indispensable to the welfare of all the rest. And the gaps which are met with in the secondary and primary rocks do not necessarily show that the case was otherwise when those rocks were in course of formation. For the presence of these gaps may have to be explained, not by supposing that certain plants and animals are missing because evolution had not then had time to do its work, but by supposing that the material of the secondary and primary rocks, organic and inorganic, had been washed away from regions which were less richly provided with plants and animals than the regions which had supplied the material for the formation of the tertiary and post-tertiary rocks. Nay it is very possible that the order of deposition might have been other than it is—that the tertiary rocks might have been first or second in the series instead of third, for in order to bring about this alteration all that is necessary is to suppose that the three regions which supplied the material for the three orders of rock were co-existent and that the change which led to the denudation of one district before or after another was timed accordingly. At all events, until these doubts are set at rest it is impossible to point to the order in which the fossils are ranged in the fossiliferous rocks, and say that countless ages must have been spent in the formation of these rocks, because countless ages are wanted for the purposes of evolution.

And do what I may, I cannot persuade myself that these doubts are in any way set at rest by the presence among the fossils of intermediate or intercalary forms which have no living representatives now. The palæotherium to a certain extent was at once horse-like, and rhinoceros-like, and tapir-like. The anaplotherium in a feeble fashion was intermediate between pigs and ruminants. The iguanadon inclines towards birds on the one hand, and towards saurians on the other: and so does the archæopteryx. But I fail to find here any evidence in favour of evolution. I see plainly enough that these intermediate or intercalary forms may be only so many additional reasons for believing that the same archetypal plan is carried out everywhere. I see nothing to make me ready to believe that these forms are transitional—that they mark the passage of one form to another by way of evolution. Nor am I inclined to think that this statement is in any way invalidated by the geological pedigree of the horse as made out from American fossils by Professor Marsh. I cannot agree with Professor Huxley in thinking that this pedigree supplies demonstrative evidence in favour of evolution. It is by no means certain that the horse of the present day and of the days corresponding in time with the formation of the post-pliocene and recent rocks may be traced back, first to the pliohippus and protohippus (hipparion) of the pliocene, then to the miohippus (anchitherium) and mesohippus of the miocene rocks, and lastly to the oldest member of the equine series yet known, the diminutive orohippus of the eocene rocks, with four complete toes on the fore limb and three on the hind limb, with well developed ulna and fibula, and with short crowned grinders of simple pattern. The differences in question are greater than those which are met with in the varieties producible in any species by domestication, or by climate, or in any other known way, and it may be going much too far to suppose that the horse is as closely related to the pliohippus, or protohippus, or miohippus, or orohippus, as it is to the ass or zebra, or quagga. The orohippus may indeed be nearly as far out of the true ancestral line of the horse as the hippopotamus. The horse is no doubt a perfect animal, but there is no reason to suppose that the orohippus was less perfect. Man is not less perfect because his limbs are not hoofed like those of the horse, and why should the eocene fossil orohippus be in a different case? In fact, I cannot bring myself to believe that any creature, plant or animal, is imperfect, and for that reason in need of evolution. I cannot look upon any of these intermediate or intercalary forms as transitional, as the outcome of evolution; I can look upon them as supplying additional illustrations of archetypal unity: this, and this only, is all I can do.

And most assuredly a safe foundation for the doctrine of evolution is not to be found in the modifications which are brought about in plants and animals by artificial and natural means, or in the embryonic development of any one of the higher animals.

Very extraordinary changes are brought about in many plants and animals by the interference of man, and in other ways, but none of them are such as to show that a lower species is perfected by being raised to the level of a higher species. In every case, so far as I know, the species themselves remain as far apart as ever; or if, in a few instances, certain closely allied species may intercross and produce hybrid forms, nature, as a rule, shows her disapproval by making these forms infertile. When left to themselves also the varieties in plants and animals which are produced by the interference of man or in other ways, and which are capable of propagating themselves in the ordinary way, are very generally in a hurry to revert to the ancestral type. The plant or animal has been shorn of some power by which it is able to live on indefinitely in a descendant exactly like itself. The garden rose has had its stamens and pistils transformed into petals: it is no longer fertile in itself: it must be propagated by the gardener artificially: and if it be not so cared for it quickly reverts to the wild type. The fancy pigeon, which has been produced by man by careful intercrossings, has lost none of its fertility, and there is no danger of extinction by reason of such loss, but if it be allowed to follow its own devices in mateing, its descendants soon don again the livery of the rock pigeon (*columba livia*), which pigeon is the common parent of all pigeons. And so with all other plants and animals whose natural growth has been interfered with artificially. Left to themselves the original state of wildness soon gains the mastery; there is an element of weakness inherent in every variety; and therefore it may be doubted whether any variety in plant or animal, however produced, is really a change for the better—is really a step forwards in the direction of evolution. And this too is the lesson which is to be drawn from the history of the rudimentary creatures at the foot of the scale of being, whose very vitality is somewhat doubtful. Bacteria, among the simplest of living units, may, as Dr. Bastian points out so clearly, be developed (possibly from inorganic elements), almost at the will of the experimenter, into monads and amœbæ and paramæciæ, or into the lowest forms of fungi—into forms of animal life, that is to say, or into forms of vegetable life: but not much is to be built upon this fact in favour of evolution. For what follows? Simply this—that instead of passing on into higher forms of being, these forms are unstable in the highest degree, and always in haste to break up again into bacteria. The tendency to retrograde is, to say the least, quite as marked as the tendency to advance: and as respects evolution, the conclusion to be drawn is even that which has been drawn from the history of the rose and pigeon—this and no other. Look indeed where I may, I see enough to show convincingly that each plant or animal has its own part to play in nature, and nothing to make me suspect that any plant or animal is not perfectly fitted for its special work. I see plainly, in fact, that any movement towards evolution anywhere must derange, and may upset, the equilibrium of nature.

And certainly I find nothing in the history of embryonic development to make me waver in this conviction. Certain passing resemblances to lower types of being are brought to light in this history, but this fact does not point to a state of things in which one creature may become another. Development always ending in the production of well defined differences is not to be confounded with evolution, even though the same path be travelled over up to a certain point. In fact the process of embryonic development has nothing to do with the process of evolution, and it is quite beside the purpose to speak of the two processes as being identical. The process of development may and does supply much additional evidence in favour of archetypal uniformity of plan in organic beings, and this, and only this, is what it does do. At all events, I do not see how the facts can be so twisted as to supply the evolutionist with conclusive evidence in favour of his own particular view.

A chief reason for believing in the extreme antiquity of the fossiliferous rocks is that which is supplied by the doctrine of evolution. Set aside this doctrine, and the reasons for entertaining a view altogether different to it retain all their force. So it may be: nay so it must be. And if so, then there is so far nothing in the history of the fossiliferous rocks to make it absolutely certain that immemorial ages have been spent in the formation of these rocks.

It is also difficult to believe that the cutting out of river-beds and river-gorges has always been a very slow process. Thus: "After the heavy rains which ſollowed the eruption of Vesuvius in 1824, the water flowing from the Atrio del Calvallo cut in three days a new chasm through strata of tuff and ejected volcanic matter to the depth of 25 feet, the chasm crossing the old mule path and making it impassable." Thus: "In many places in Georgia and Alabama land floods have given rise to recent ravines from 70 to 80 feet in depth in tertiary and cretaceous formations." Sir Charles Lyell, whose words are here quoted, also says, "When we are speculating upon the excavating power which a river may have exerted in any particular valley, the most important question is, not the volume of the existing stream, nor the present level of the channel, nor even the nature of the rocks, but the probability of a succession of floods at some period since the time when the valley may have been first elevated above the sea." What has had to be done may in fact have been done in a comparatively short period, even in such a case as that of the gorge below the falls of Niagara. For instead of believing that the falls have formed the gorge by eating their way backwards from the escarpment above Queenstown, where they were originally, to their present position, a distance of seven miles, either at the rate of one inch a year for 35,000 years, or at the more rapid rate of three inches a year for a third of this period, I find it more easy to believe that in the first instance the river found its way to the escarpment at a much higher level, in a bed of which there are distinct traces not far from the top on each side of the gorge, and that, afterwards, the gorge was formed, either suddenly, under the tearing-action of some terrible earthquake or flood, or else less suddenly, under the grinding-action of the glacier which covered the district for some time after the advent of the present state of things as to climate. Indeed, do what I may, I can only allow that a very small part of the ravine now existing has been scooped out by the river slowly creeping backwards at the falls.

And nothing to the contrary is to be found in the important diggings and borings carried out under the direction of Mr. Horner at Heliopolis and Thebes (Phil. Trans., 1855 and 1858), with a view to ascertain the rate at which the alluvium of the Nile is deposited. The depth of the alluvium, or the soil which is regarded as alluvium, under which a monument of known date is buried, supplies the gauge; the particular case on which the argument is chiefly based being that of the partially buried pedestal once under the fallen colossal statue of Rameses II. at Memphis. Over the base of the pedestal lie 9 feet 2 inches of alluvium; under it are 36 feet of the same sort of soil. These facts, which are brought to light by the diggings and borings, are supposed to show that the Nile has been throwing down mud here for 13,000 years at the rate of 3·5 inches in a century, because this is assumed to be the rate at which the base of the pedestal has been buried in the years which have gone by since the erection of the statue in 1361 B.C. But in objection to this conclusion it may be stated, as was done by Mr. Samuel Sharpe, that the Egyptians were in the habit of protecting their temples and monuments from the annual inundations of the Nile by enclosing them with dykes, and that the mud in which this particular pedestal is buried only began to accumulate at the doubtful date when the dyke around it was allowed to get out of repair. And this objection cannot be set aside as frivolous, for, by being thus protected, many of the temples and monuments which were seen by Herodotus were in hollows considerably under the level of the surrounding ground. Moreover, the conclusion drawn by Mr. Horner in this particular case is not borne out by the results of other diggings and borings, nineteen in number, carried out by himself at the same time in the same district, some of them in close proximity to the statue. Sooner or later at different depths the work of digging and boring was always stopped by the influx of water, the underlying rock being reached in no single instance. The soil taken out was of two principal sorts, an argillaceous earth or loam more or less mixed with sand, which is regarded as Nile sediment or mud, and quartzose sand which is called desert sand. Sometimes the quartzose sand was absent, sometimes it was below the argillaceous loam, once, with the exception of a thin layer of loam at the top, there was nothing else but it, and in three or four instances the column of loam was broken in more than one place by layers of this sand, some of them of considerable thickness. Evidently the quartzose sand could not be looked upon as a foundation for the loam; evidently the winds of the desert, sand laden, have brought sand to the spot at irregular intervals; and as evidently all the argillaceous loam cannot be looked upon as sediment from the waters of the Nile, for as M. Malley (quoted by Mr. Horner) points out, "Pendant trois ou quatre mois de l'année la surface de l'Égypte, dénuée de végétation, sèche et poudreuse, est balayée par des vents violents, qui soulevent dans les airs la poussière du sol, en laissant précipiter une partie dans le fleuve qui l'entraine à la mer, et en dispersant une autre dans les déserts, ou en l'accumulant sur d'autres portions de l'Égypte." It is indeed certain that some districts may have been bared and others buried in this way; and therefore, instead of supposing that the depth of the soil in which the pedestal of the fallen statue of Rameses II. is buried is a gauge by which to measure the rate at which the alluvium has accumulated since the erection of the statue in 1361 B.C., it may be that this gives only a small portion of this depth. And besides all this, it is not improbable that a portion, perhaps a large portion, of the deep seated soil of the Nile valley may have been deposited, not from the waters of the Nile after the establishment of the present state of things, but from the waters of the sea while Egypt was a shallow marine estuary. At all events, in the case under consideration, there is nothing to show conclusively that the Nile has been depositing mud upon the land for 13,500 years.

And not more conclusive, as it seems to me, is the evidence which is thought to point to the existence of man upon the earth long before this period. Human bones, with works of art and several other indications of the presence of man, are met with in the post-tertiary (quarternary or pleistocene, and recent) formations, namely, loam, and loess of various sorts, fresh water gravel and alluvium, peat mosses, sand dunes, and other superficial deposits; and, from the order in which these relics are deposited, there is reason to speak of an age of stone as having been followed by an age of bronze, and of an age of iron as coming last of all. But it does not follow that the first of these ages reaches back to immemorial times, or that the last, which has not yet come to an end, had its beginning very long ago. It is even possible that Herodotus, if he had happened to wander in the right direction, might have had something to say about the men who lived in lake dwellings, or who left behind them such large kitchen middens, or dust heaps, as well as about the men belonging to the so-called bronze and iron ages whose remains are to be found in so many peat-mosses, and sand dunes, and barrows. And so with the relics of the same sort which are met with in beds of fresh water gravel and alluvium now many feet above the level of the river which was concerned in forming them—the beds which supply the chief data for supposing that man must have lived upon the earth for ages well nigh uncountable. There is, for example, no certain evidence that man existed very many ages ago in the traces of humanity which are met with in the terraces of loam and river gravel raised high up above the present level of the Somme near Abbeville. These traces would supply such evidence if the valley had been scooped out by the river at the slow rate of erosion now going on, but it is by no means certain that the valley was formed in this manner. On the contrary, it is quite possible that the valley may have been formed, not slowly by the simple wash of the river, but quickly, either by the outburst of pent-up waters, or by the overwhelming grinding of the glaciers which would seem to have covered the district in the glacial epoch which ushered in the present state of things as to climate. The earlier deposits containing articles of palæolithic manufacture, with the bones of the mammoth, the hairy rhinoceros, and other extinct animals, and with, in addition, a human bone here and there, as well as the later deposits containing articles of neolithic manufacture, with various other rude works of art, together with the bones of dogs, and hogs, and horses, and sheep, and short-horns (of domesticated animals, that is to say, which do not belong to the indigenous fauna of Europe, and which may have been brought from Central Asia by a higher race of man in some migration), and with several kinds of grain and fruit-seed, also not indigenous, and with many bones of man, are all found in the post-tertiary formations which are contemporaneous with the glacial times which ushered in the present state of things as to climate; and all that can be said with certainty at present is that man lived upon the earth in these glacial times, and that these times may after all be comparatively recent. At all events it is not difficult to explain the presence of man at the time when the terraces of gravel and loam in the valley of the Somme near Abbeville were formed high up above the present level of the river, without supposing that man lived in this valley or elsewhere in immemorial times.

In the stratification of the present land it is evident that this land was formed under water, and raised into its present position afterwards, but it is not so evident that the land generally has been in and out of the water several times. This up and down movement may have happened more than once in the case of some of the coal-seams; in all probability it did so happen; but after what has been said about the action of drifts in the formation of these seams, it is difficult to believe that coal often marks the site of an ancient forest which had lived and died on the spot. Indeed, so far as I can see, the evidence generally points towards quickly forming drifts rather than towards slowly growing forest or scrub. And as in this case, so in that of the strata built up in the main of the remains of animals which are supposed to have lived and died on the spot, all the facts may be so read as to convey the same lesson. Nor can it be said that there is anything in the remarkable contortions of many of the strata to show that the strata have been in and out of water several times. For the most part all the strata, contorted or not, as the case may be, lie conformably one above the other in the same order, and on looking into the matter with any degree of care, it seems to follow almost as a matter of course that all the strata may have been formed without any break in the same period of submersion. And with respect to the duration of this period, all I can say is that I am more than ever disposed to shorten it, for in addition to what has been already said in opposition to the notion that interminable ages have been spent in the work of stratification, it remains to say that the ancient earth may have been much looser in contexture than the present earth, and that, for this reason, the formation of the strata of the present earth, as far as the inorganic constitutents of the strata is concerned, may have been greatly facilitated. Indeed, I cannot help fancying that the rocky character of the present earth is in the main attributable to the action of the heat and pressure which were brought to bear upon the strata from below when the bed of the ancient sea was converted into the present land. And, in short, if anyone were to say that the fossiliferous rocks had been formed in historic times, I should not venture to contradict him.

Nor do I want much time in order to account for the cold which ushered in the present state of things. All I want in the first instance is the time necessary to freeze the waters of the sea, as sooner or later they must have been frozen, in a state of universal deluge, and, so far as I can see, a few months at most will serve for this. For after what has been said it may be supposed that the great revolution in which the bed of the ancient sea was converted into the present land, may have been completed in a single year, and that the state of universal deluge which would happen when this revolution was midway between the beginning and the end, would be comprised within this short period. And all I want afterwards is time enough for the tremendous glaciers which in the first instance covered the present land in a way in which it is not covered now to melt away—a time which would not and could not be very long, because the sun would be then acting upon the earth as at present. Indeed, I find no occasion to call in the help of ice-epochs connected with many precessions of the equinoxes and many revolutions of the apsides, each of them lasting thousands upon thousands of years, and each separated from the rest by thousands upon thousands of years, in order to account for the cold of arctic severity which ushered in the present state of things as to the climate.

And, finally, I do not find it necessary to believe that countless ages have been spent in the formation of the igneous and other unstratified rocks which underlie the fossiliferous rocks. I cannot bring myself to believe that these rocks were once in a state of fiery mist, and that ages without number had to pass by before they became solidified by cooling. I can easily believe that the universe was cold, and dark, and probably motionless, before the day when the various orbs in space, as *bodies*, began to interact electrically as they do now, and that this day, instead of being immeasurably distant, may only mark the commencement of historic time. And I am, I think, much the gainer by thus changing the point of view from heat to electricity. I can see how electrical repulsion may do the work of heat in keeping molecules apart. I can see how electricity may pass from the statical into the current state if only the interacting bodies in which the electricity originates are of different degrees of potential, the current passing from the body at the higher potential to the body at the lower. I can see how current electricity may be frittered down into heat by resistance, and how there may be no heat when no resistance is opposed to the passage of the current. I do not want rays of heat; I only want electrical force moving in lines between two or more bodies, and the movement impeded by a certain degree of resistance where heat is developed, and there only. And I can even imagine that heat as heat has its seat only in the nervous apparatus which takes cognizance of heat. And as with heat so with light, I can see that there may be the same electrical basis, and that electricity may be frittered down into light as well as into heat by resistance; for the fact that the velocity of light is much less than that of electricity may help to show that light is electricity bridled down by resistance. I do not want rays of light; I can do without luminiferous ether; I only want the electrical relations between different bodies which are exhibited in the laboratory, and liberty to believe that the lines of electrical force between two or more bodies (which lines imply the existence everywhere of a medium which is perhaps a shade less subtle than luminiferous ether) are bright where they are frittered down into light by resistance, and dark elsewhere. Indeed, as with heat so with light, I can imagine that light as light has its seat only in the apparatus of vision. Nay, I can dimly see how a key for gravitation and for tangential movement may be formed by seeking for it in the very same lines of electrical force which serve as a basis for heat and light. I can see how gravitation may be nothing more than the movement in the current of a body at a higher potential to a body at a lower potential. I can see how the tangential movement, which may have to act as continuously as gravitation, and which must so act, if, as may well be, space is pervaded with a medium capable of opposing any degree of resistance to the movements of the earth and heavenly bodies, may be nothing more than the movement of a body in the magnetical current which passes across the electrical current—which passes, it may be, not around this latter current, but across it. By giving rein to my thoughts in this manner, I become more and more unbelieving in the "nebular hypothesis," and in the consequent notion that countless ages have been spent in the formation of the igneous and other unstratified rocks underlying the fossiliferous rocks, and more and more inclined to believe that the universe was cold and dark and forceless as well as formless before the day when the various orbs in space were formed and placed in position, and when they began, *as bodies*, to interact electrically as they do now, and that this day, instead of being immeasurably distant, may only mark the commencement of historic time.

SUB-CHAPTER VI.

IN WHICH IT IS SUGGESTED THAT THERE MAY BE SOMETHING PROPHETIC AND NOTHING REALLY UNSCIENTIFIC IN THE BIBLICAL HISTORY OF THE EARTH AND HEAVENS.

THE guiding voice to which I am compelled to listen when I try to understand what is told in Genesis about the past history of the earth and heavenly bodies is not that which gives the word of command to the scientific train-bands in the regulation uniform of the present day. It does not tell me to take part on either side in the brilliant controversy in which Mr. Gladstone and Mr. Huxley have been recently engaged. It does not tell me to try to find an account of the days of creation in the "records of the rocks," or to pin my faith either to the "nebular hypothesis," or to the doctrine of organic evolution. It tells me to believe that the universe was cold and dark and forceless as well as formless before the day when the earth and the heavenly bodies were formed and placed in position, and when they began, *as bodies*, to interact electrically as they do now, and simply to read the words of Scripture from this point of view. In a word, it tells me to go on in the way in which I have been travelling hitherto, and to hope that in due course I shall arrive at a satisfactory solution of the problem with which I have now to do.

This problem is stated very clearly.

"In the beginning God created the heaven and the earth. And the earth was without form and void; and darkness was upon the face of the deep. And the Spirit of God moved upon the face of the waters. And God said, Let there be light: and there was light. And God saw the light that it was good: and God divided the light from the darkness. And God called the light Day, and the darkness He called Night. And the evening and the morning were the first day.

"And God said, Let there be a firmament in the midst of the waters, and let it divide the waters from the waters. And God made the firmament, and divided the waters which were under the firmament from the waters which were above the firmament: and it was so. And God called the firmament Heaven. And the evening and the morning were the second day.

"And God said, Let the waters under the heaven be gathered together unto one place, and let the dry land appear: and it was so. And God called the dry land earth; and the gathering together of the waters called He seas: and God saw that it was good. And God said, Let the earth bring forth grass, the herb yielding seed, and the fruit tree yielding fruit after his kind, whose seed is in itself, upon the earth: and it was so. * * * And God saw that it was good. And the evening and the morning were the third day.

"And God said, Let there be lights in the firmament of the heaven to divide the day from the night; and let them be for signs, and for seasons, and for days, and for years: And let them be for lights in the firmament of heaven to give light upon the earth: and it was so. And God made two great lights; the greater light to rule the day, and the lesser light to rule the night: He made the stars also. And God set them in the firmament of the heaven to give light upon the earth, and to rule over the day and over the night, and to divide the light from the darkness: and God saw that it was good. And the evening and the morning were the fourth day.

"And God said, Let the waters bring forth abundantly the moving creature that hath life, and fowl that may fly above the earth in the open firmament of heaven. And God created great whales, and every living creature that moveth, which the waters brought forth abundantly, after their kind: and every winged fowl after his kind: and God saw that it was good. And God blessed them, saying, Be fruitful, and multiply, and fill the waters in the seas, and let fowl multiply in the earth. And the evening and the morning were the fifth day.

"And God said, Let the earth bring forth the living creature after his kind, cattle, and creeping thing, and beast of the earth after his kind: and it was so. * * * And God saw that it was good. And God said, Let us make man in our image, after our likeness: and let them have dominion over the fish of the sea, and over the fowl of the air, and over the cattle, and over all the earth, and over every creeping thing that creepeth upon the earth. So God created man in His own image, in the image of God created He him; male and female created He them. * * * And God said, Behold, I have given you every herb bearing seed, * * * and every tree in the which is the fruit of a tree yielding seed; to you it shall be for meat. And to every beast of the earth, and to every fowl of the air, and to everything that creepeth upon the earth, wherein there is life, I have given every green herb for meat, and it was so. And

God saw everything that He had made, and, behold, it was very good. And the evening and the morning were the sixth day.* * *

"And God blessed the seventh day, and sanctified it: because that in it He had rested from all His work which God created and made."

A story this, aflame with poetic fire from beginning to end, but not, so far as I can see, either inconsequent or incredible.

"In the beginning God created the heaven and the earth. And the earth was without form and void; and darkness was upon the face of the deep." The story so far is quite consistent with the notion that the universe may have been cold and dark and motionless before the day when the earth and heavenly bodies were formed and placed in position, and when, *as bodies*, they began to interact electrically as they do now. Nay it may have been empty also.

"And the Spirit of God moved upon the face of the waters." Waters! what waters? Not waters on the surface of the earth simply: that is evident. The firmament which divided the waters from the waters is not the terrestrial dry land, for the dry land did not appear until the morning of the third day: and hence the waters in question must mean more than separate seas. The firmament is not the atmosphere, and the waters above it the clouds, as Mr. Ruskin imagines, for the firmament, which is called heaven, is afterwards described as the place where the sun and moon and stars are set for signs, and for seasons, and for days, and for years. And so it may be that the *waters* above and below the firmament are the sun and moon and stars on the one hand, and the earth on the other, all these bodies being in fact covered with water in the first instance. But be this as it may, there is no room for doubt as to what was the condition of the earth at this time. Up to the third day this was that of a shoreless ocean. Morning twice dawned and evening twice fell upon a land-less waste of water. On the third day, and not until then, "the waters under the heavens were gathered together into one place, and the dry land appeared." As to the meaning of the words there can be no mistake.

"And the Spirit of God moved upon the face of the waters. And God said, Let there be light: and there was light. * * * And God divided the light from the darkness. And God called the light Day, and the darkness Night. And the evening and the morning were the first day." The light was the light of day, for it was separated from the darkness, which was night, and therefore the earth must have been rotating on her axis from the first, light and motion, of this sort at least, then going hand in hand as now. Nor is there anything contradictory to this conclusion in the statement that the *lights* which were to divide the day from the night, and to be for signs, and for seasons, and for days, and for years, did not appear until the fourth day. The heavenly bodies were not created on this day. They appeared as *lights* on this day. Until then these *lights* may have been hidden by the mist "which went up from the earth and watered the whole face of the ground," as it did on the day when the earth was covered with vegetation, which day was the third day. Or in the tremendous commotion attendant upon the rising of the land above the water the air may have been so much overcast with dense clouds of watery vapour as to make the lights which were the source of light invisible. There is nothing inconsequent in the story so far.

The separation of light from darkness is very significant. The light is the light of day, the darkness is night. The earth is evidently rotating on her axis at the time. The light is connected with motion, and it may also be connected with electricity and magnetism and heat, the connection being that which is set forth in the Doctrine of the Correlation of the Physical Forces so very conclusively. And in the rising of the land above the water which soon follows upon the creation of light it is also possible to find further evidence of the connection between light and heat, and between light and another manifestation of motion, for, according to the premisses, this rising is caused by the heat of the ring of subterranean fire of which the development is concurrent with that of light. Indeed, there is nothing in these statements which is inconsistent with the belief that the universe was cold and dark and motionless before the day when the earth and heavenly bodies were formed and placed in position, and when they, *as bodies*, began to interact electrically as they do now. And without irreverence it is even possible to go further and believe that the physical way in which the Spirit of God moved upon the waters may be no other than that which may be said to open out in the Doctrine of the Correlation of the Physical Forces.

Plants of all kinds are created before animals, and animals of all kinds before man, and of them it is said, "and God saw everything that He had made, and, behold, it was *very good.*" There is nothing to support the doctrine of evolution from a state of imperfection, with one most rudimentary form as the starting-point for all plants and animals and man, or with a few such points. Creation is carried out speedily, and in a given order, and why not? Innumerable ages are wanted for the purposes of evolution, but not so with creation. Indeed the wonder is, if there be room for wonder, that the Creator, with whom a thousand years are as one day, and one day as a thousand years, should have spent so long a time as six days in doing what is said to have been done on these days. But it does not follow that the living creatures which thus sprang into existence at the word of command on the days of creation should be unaffected by evolution. On the contrary it may be allowed that they respond to the modifying action of natural causes to a very considerable extent, though not to the extent which would satisfy an ultra-evolutionist. It is certain that many plants and animals now existing differ very decidedly from the fossil plants and animals of which they are the lineal descendants. It is highly probable that important *varieties* have been produced in the course of time, and that "the struggle for existence" and "the survival of the fittest" may have largely contributed to this result. But nowhere do I find any sufficient reason for believing that variations in any one species have ever been carried so far as to obliterate the essential differences between it and any other species, or genus, or order, or class. Nor do I find anything unscientific in what is said about the work done on the days of creation, or about the vegetable diet of man and animals in the first instance, or about the pre-eminence of man. The arrangement of the fossils in the fossiliferous rocks, be that what it may, can, as I believe, have nothing to do with the days of creation, and, therefore, this to me does not supply a reason for saying that the work said to be done on the days of creation was not done on these days. Indeed, so far as I know, there is no reason why the account of this work may not be taken simply as a narrative which has nothing to do with science. And it is not altogether incredible that man himself and all animals should have found their food in the plant-world in the first instance, for the very existence of herbivorous animals is in itself a sufficient proof that the materials necessary for the formation of flesh are met with within this world. And, as is well known, there is much to be said in favour of the pre-eminence of man—but this topic I must pass by as one which is too large to be dealt with incidentally in this place.

And with the same key it is also possible to pass on far enough to be able to see in the story of the deluge a continuation of the story of the creation. How strangely circumstantial is this story! This catastrophe is slow and orderly, as if the causes at work were natural causes: it was also predicted—as an eclipse might have been. "In the second month, the seventeenth day of the month, the same day were all the fountains of the great deep broken up, and the windows of heaven were opened. And the rain was upon the earth forty days and forty nights. * * * And the flood was forty days upon the earth. * * * And the waters prevailed exceedingly upon the earth: and all the high hills, that were under the whole heaven, were covered. Fifteen cubits upward did the waters prevail; and the mountains were covered. * * * And the waters prevailed upon the earth an hundred and fifty days. * * * And the waters assuaged. The fountains also of the deep and the windows of heaven were stopped, and the rain from heaven was restrained. And the waters returned from off the earth continually: and after the end of the hundred and fifty days the waters were abated. And the ark rested in the seventh month, on the seventeenth day of the month, upon the mountains of Ararat. And the waters decreased continually until the tenth month; in the tenth month, on the first day of the month, were the tops of the mountains seen. * * * And in the second month, on the seven and twentieth day of the month, was the earth dried."

And so it may well be if the deluge was brought about in the way to which I have directed attention, that is by the tilting of the axis of the earth from a position in which it was perpendicular to the plane of the ecliptic into the position it now occupies, and by the changes in the development of subterranean heat consequent thereon. The assumption is that the ring of subterranean fire is at its maximum degree of development under the regions upon which the rays of the sun and moon fall directly,—that before the tilting the development of subterranean heat and the raising of the land consequent thereon were for this reason chiefly under and in equatorial regions,—that after the tilting the parts of the earth exposed to the direct action of the solar and lunar rays in the course of the year would widen until they included all the parts between the tropical lines,—that the part upon which the solar and lunar rays fell directly immediately after the tilting may have been as far away from the equator as either tropical line, a distance of more than 1,500 miles,—that this part may then have been covered with sea,—that, for this reason, in addition to the ring of subterranean fire under the equator, a second ring may have been formed under the solstitial line,—and that, afterwards, as the days wore on, other rings would form so as to fill up the space, first between this solstitial line and the equator and then between the equator and the other solstitial line,—that the bed of the sea may have been raised by the heat of the rings of fire formed under it,—that eventually the bed of the ancient sea may have been raised until it became the present land,—and that, when half accomplished, this raising of the bed of the ancient sea may have led to a state of universal deluge by forcing the water over the ancient land. And most assuredly it is not difficult to see that the biblical statements are not a little simplified and elucidated by adopting this assumption.

The fountains of the great deep are broken up and the windows of heaven opened on the seventeenth day of the second month, that is—the Jewish year beginning on the 20th of September—on the 6th of November. For forty days and forty nights, that is, until the 16th of December, which is very near the time of the solstice, the waters go on rising, and then, after prevailing for one hundred and fifty days so as to cover the highest hills to the depth of fifteen cubits, they begin to subside. The ark grounded on the top of Ararat on the seventeenth day of the seventh month, that is on the 27th of March: the tops of the mountains are seen on the first day of the tenth month, that is, on the 20th of June, the day before the solstice: the earth is dried on the 27th day of the second month, which is the 16th of November. The catastrophe occupies very nearly an exact year, beginning and ending in November at dates only ten days apart.

The fountains of the great deep are broken up and the windows of heaven opened on the 6th of November, and rain pours down in torrents for forty days and forty nights. The words do not merely point to the land as being flooded under a mighty downpour of rain: they also point to a breaking up of the fountains of the great deep, which may mean that the ancient land was mainly submerged by a raising of the bed of the ancient sea, which raising began on the 6th of November, which went on for a year, and which, in accordance with the premisses, may have been caused by the axis at the earth having been tilted on this day from its original position into that it now occupies.

The rain was upon the earth forty days and forty nights. It would seem as if, in causing the deluge, the rain played an insignificant part in comparison with that played by the raising of the bed of the sea. It would even seem as if the rain, which went on for forty days and forty nights, ceased at the end of this time. And why? Did the sea overlying the rising land during these forty days and forty nights, boil under the heat proceeding from the land, and may rain have fallen in torrents during this time in consequence of the sky being overloaded with steam? Did the rain cease at the end of these forty days and forty nights, because, owing to the land being then wholly or in great measure under water, the reign of ice had commenced by which the ice-caps of the poles were eventually drawn over the earth until their edges were on the point of meeting at the line—a state of things which might very well prevent the formation of rain. I see no reason why it may not have been so; indeed, after what has been said, this conclusion would almost seem to be that which follows naturally from the premisses.

Nor is there anything inconsequent in the remainder of the story of the deluge. On the contrary, after what has been said, it is easy to go on far enough to see, not only that this story is consistent with itself from beginning to end, but also that, in addition to describing the deluge, it may also describe how the bed of the ancient sea was converted into the present land. Nay it is not difficult to find what would seem to be a crucial reason for believing at once in the truth of the biblical story of the deluge and in the truth of the premisses in the fact that the time of which the story takes account, a year only, is the very time which is required by the premisses to bring about the same result.

And it may also be quite in order that there should have been no rain and no rainbow before the time when the fountains of the great deep were broken up, and the windows of heaven opened. There are regions of the earth now in which the stay-at-home inhabitants know nothing of rain or rainbow except by report, and it is not at all unlikely that the inhabitants of the antediluvian world may have been in like case. Before the deluge there may have been no land to speak of except in equatorial regions. Before the deluge the land may have been rainless and bow-less, because it was always hyper-tropically hot. Before the deluge, indeed, the vapour arising from the hot land, which now may fall as rain close at hand, might rise into the air high enough to meet with the currents which would carry it polewards, the case being one in which, whether it fell as rain or hail or snow, it would fall far out of sight of land. Nor is a fatal objection to this view to be found in the "rain-prints" left on the slab of arenaceous green shale which was found at Cape Breton in Nova Scotia—a slab which belongs to the carboniferous system of rocks. It is certain that this slab was formed long before the revolution which converted the bed of the ancient sea into the present land: it is not certain that the rain-prints so-called were really caused by rain. Along with them are marks which are evidently worm-tracks. The slab, almost as evidently, was once a muddy shore where the worms which left these tracks had their dwelling, and who can say with certainty that the rain-prints so-called may not have been caused by drops from a breaking wave, or from a waterfall close at hand? There would be no rainbow proper in this case. At all events, it is not possible to say that the absence of rain and rainbow in antediluvian times is altogether inconsistent with the premisses.

What then? Am I at liberty to accept the biblical history of the earth as the actual history? Am I to believe that the stratified rocks were formed in the comparatively short interval between the creation and the deluge? Am I to believe that in the great revolution in which the bed of the ancient sea was lifted up until it took the place of the present land there was an intermediate stage in which this deluge happened, as a matter of course, in the way pointed out? Am I to believe that the signs of glacial action, including the great denudations of the chalk and the tertiary strata, together with the scooping out of wide and deep valleys, point to a comparatively short period in which the earth was ground down by the tremendous glaciers which were called into existence by the cold of the deluge, and which continued to act until the melting of the ice over the new land led to the introduction of the present state of things as to climate? There can be no uncertainty in the answers which ought to be returned to these questions if there be no flaw in the argument. It can only be *yes.* And, certainly I have yet to learn why, on good grounds, *no* should be put in place of *yes* by any one, and why what I have ventured to say may not be regarded as offering at least "a working hypothesis."

WORKS BY THE SAME AUTHOR.

VITAL MOTION

AS A MODE OF PHYSICAL MOTION.

BY

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> "Le nom de *Galvani* ne périra point; les siècles futurs profiteront de sa découverte, et, comme le dit *Brandes*, 'ils reconnaîtront que la physiologie doit à *Galvani* et à *Harvey* ses deux bases principales.'"
>
> ALEXANDER VON HUMBOLDT.

Second Edition, *8vo.*, 7/6. *Macmillan & Co.*, 1882.

CONTENTS.

THE ARGUMENT.

The view here taken is that the sheaths of the muscular fibres while alive and at rest are so many charged Leyden jars, that this charge inhibits or antagonizes the state of muscular contraction by keeping the muscular molecules in a state of mutual repulsion, and that contraction happens when this charge is discharged, because the muscular fibres are then given over to the attractive force which is inherent in the physical constitution of the muscular molecules. The discharge analogous to that of the Torpedo, which accompanies muscular contraction, and which appears to be only the faradaic current which attends upon any sudden movement of electricity, is supposed to produce muscular contraction, not by acting as a *stimulus* to a vital property of contractility, but simply by discharging the charge which inhibits contraction, and which returns the moment the muscle is left to itself. But not so with rigor mortis, for here the muscle is supposed to stiffen slowly and permanently, simply because the charge which keeps up the state of muscular relaxation departs slowly and does not return. And all that may be said about muscle may also be said about nerve, except this, that in nerve, probably for the same reason as that which puts an end to amœboid movements in granular amœbæ, the state of action is not accompanied by contraction. The case indeed is altogether opposed to the notion that muscle and nerve are endued with a vital property of irritability, and that muscular contraction and nervous action are brought about by the excitation or stimulation of this property, for everything goes to show that the so-called stimuli, electricity, nervous influence, blood, and the rest, keep up a state which inhibits or antagonizes the state of action in muscle and nerve, that convulsion, and spasm, and tremor, and painful sensations of a neuralgic character, are signs, not of hyper-vitalization, but of de-vitalization, and that, in fact, a great revolution is needed in the treatment of all the disorders in which the muscles and nerves are prone to pass from the state of rest into that of action.

PROPOSITIONS.

I.

That the fibres of living nerve or muscle, while at rest, are charged more or less positively at any point of their sides, and more or less negatively at any point of either of their two ends.

II.

That the charge which is present in living nerve and muscle during the state of rest disappears in great measure when this state changes into that of action.

III.

That the state of action in nerve and muscle is accompanied by a discharge analogous to that of the Torpedo.

IV.

That there are in dead nerve and muscle no traces of the electrical phenomena which are peculiar to living nerve and muscle during the state of rest.

V.

That the natural electricity of a nerve is affected very differently by anelectrotonus and by cathelectrotonus, anelectrotonus augmenting, preserving and even re-calling it, cathelectrotonus diminishing and speedily destroying it.

VI.

That the natural activity of a motor nerve is affected very differently by anelectrotonus and by cathelectrotonus, anelectrotonus suspending, preserving, and even re-calling it, cathelectrotonus, after exaggerating it for a short time, speedily destroying it.

VII.

That a motor nerve is paralysed by cathelectrotonus as well as by anelectrotonus if the voltaic apparatus used in producing the electrotonic state be sufficiently powerful.

VIII.

That a motor nerve is delivered for the moment from the paralysing action of electrotonus by touching it with a conductor, but not by touching it with a non-conductor.

IX.

That a motor nerve is prevented from acting in electrotonus by the artificial charge which is communicated to it from one or other of the voltaic poles, and that the state of action is recalled when this charge is discharged.

X.

That a motor nerve or muscle may be prevented from acting by its natural charge of electricity, and that the state of action may be brought about by the discharge of this charge.

XI.

That the charge which prevents the state of muscular contraction may act by keeping the central muscular molecules in a state of mutual repulsion, and that the discharge which permits the state of muscular contraction may act by leaving the muscular molecules free to yield to the attractive force which is inherent in their physical constitution.

XII.

That the chief if not only difference between ordinary muscular contraction and rigor mortis may be in this—that the charge which prevents contraction departs suddenly and returns immediately in the former case, and that it departs slowly and stays away permanently in the latter case.

XIII.

That the movements of the needle of the galvanometer which are characteristic of electrotonus may be due to the discharge through the coil of the charge which proceeds from one or other of the voltaic poles, the discharge being bridled down into the quieter pace of the constant current by the resistance which it encounters in the coil.

XIV.

That electricity supplies a key to the electrotonic alternations in the natural activity of nerve which are brought about by transposing the voltaic poles.

XV.

That there is a strict parallelism between the changes brought about in the natural activity of motor nerve by the action of anelectrotonus and cathelectrotonus and the changes brought about in the same activity by the action of the so-called *inverse* and *direct* voltaic currents.

XVI.

That the changes in the motility of a galvanoscopic frog which are commonly referred to the voltaic current being *inverse* in one limb and *direct* in the other are really due, not to this cause, but, as in electrotonus, to the charge associated with the current being positive in one case and negative in the other.

XVII.

That in the case where a state of action is set up in a galvanoscopic frog by voltaic electricity the explanation has to be sought, not in the working of the constant current, but in that of the instantaneous faradaic currents which traverse the voltaic circuit at the moments of closing and opening it.

XVIII.

That the apparently eccentric behaviour of the muscles of a galvanoscopic frog under the action of the *inverse* and *direct* voltaic currents is found to be strictly in order when due heed is paid, to the differences in the direction and strength of the faradaic currents which attend upon the closing and opening of the voltaic circuit, to the gradual loss of muscular activity, to the differences between nerve and muscle as to electric conductibility, and to the loss of the capacity for reflex action which takes place presently.

XIX.

That the last proposition is not contradicted by what happens when the undivided sciatic nerve of a rabbit is exposed to the action of the inverse and direct voltaic current, for here it is found that the cry, which betokens sensation, like the contraction in the muscles supplied by the sciatic nerve, must be referred not to the working of the constant current, but to the working of the faradaic currents, one or both, which attend upon the beginning and ending of the constant current, and that the key to sensation and motion in this case is one and the same.

XX.

That faradaic currents may bring about the state of action in nerve and muscle by discharging a charge which kept the nerve and muscle in a state of rest.

XXI.

That a nerve or muscle which is thrown into a state of action by franklinic electricity is so thrown, not by the charge, but by the discharge.

XXII.

That the state of action may be marked by contraction in muscle and unmarked in nerve, not because the muscle and nerve are endowed with two different sorts of vital irritability, but simply because the material of muscle and nerve may differ in very much the same way as that in which an amœba which is capable of moving is found to differ from an amœba which is incapable.

XXIII.

That muscle may lengthen and shorten without undergoing any change of volume, not because it is endowed with a peculiar vital property of contractility, but simply because it is an elastic body.

XXIV.

That the force of the contraction may lessen as the muscle shortens simply because the muscle is an elastic body.

XXV.

That it is not necessary to entertain the hypothesis that a living power of contractility has been destroyed by death in order to account for the diminished cohesiveness of dead muscle.

XXVI.

That a muscle may in time become incapable of contraction, not because a vital property of contractility is too much fatigued to allow of further contraction until it has been *revived* by rest, but simply because the electromotive apparatus of the nerve and muscle has got out of gear by being kept in action too long.

XXVII.

That there is no reason to look upon the contractile force of muscle as a product of the transformation of the natural electricity of muscle.

XXVIII.

That the action of so-called "stimuli" in causing vital motion does not necessarily imply the "irritation" of a vital property of irritability.

XXIX.

That voluntary muscular action has nothing to distinguish it from other forms of muscular action so far as its electrical history is concerned.

XXX.

That there are many cogent physiological reasons for believing that the disposition to vital motion in nerve and muscle is inversely related to the supply of blood to the parts concerned in the movement, and that the blood acts, not as a stimulus to a vital property of irritability, but through the instrumentality of natural electricity.

XXXI.

That there are several physiological reasons for believing that the disposition to vital motion in nerve and muscle is inversely related to the supply of "nervous influence" to the parts concerned in the movement, and that "nervous influence" acts, not as a stimulus to a vital property of irritability, but through the instrumentality of natural electricity.

XXXII.

That during the state of rest every cell and fibre of nerve and muscle may react electrically with every other cell and fibre of nerve and muscle and help in keeping up the state of molecular tone or tension which opposes or inhibits the state of action, and that there is no occasion to believe in the separate existence of an inhibitory system of nerves.

XXXIII.

That the physiological history of rhythmical vital motion is in strict accordance with the previous conclusions respecting the action of blood and "nervous influence" in ordinary vital motion.

XXXIV.

That an amœba or amœboid body may move as it does move merely because, in obedience to the continual fluctuations in atmospheric potential, its irregularly distributed hyaline and mobile portions swell and shrink to a degree which is far beyond that to which its irregularly distributed granular and comparatively immobile portions are capable of reaching.

XXXV.

That vibratile cilia may move as they do simply because they are acted upon by the instantaneous faradaic currents which are induced in and around them by the movements which are continually happening in atmospheric potential.

XXXVI.

That the history of epilepsy and other forms of convulsion, as set forth in the condition of the vascular and nervous systems, is in flat contradiction to the notion that convulsion is caused by an excessive development of "nervous influence" consequent upon an over-abundant supply of blood.

XXXVII.

That the history of tetanus and other forms of spasmodic disorder, as set forth in the condition of the vascular and nervous systems, is strictly in accordance with the history of convulsion, and with the physiological premisses.

XXXVIII

That the history of tremor in its various forms, as set forth in the condition of the vascular and nervous systems, is strictly in accordance with the history of convulsion and spasm, and with the physiological premisses.

XXXIX.

That the history of neuralgia in its various forms, as set forth in the condition of the vascular and nervous systems, is strictly in accordance with the history of convulsion and spasm and tremor, and with the physiological premisses.

PROTEUS,

OR

UNITY IN NATURE.

„Die Geiſterwelt iſt nicht verſchloſſen;
Dein Sinn iſt zu, dein Herz iſt todt!
Auf, bade, Schüler, unverdroſſen
Die ird'ſche Bruſt im Morgenrothe."
Goethe (Fauſt).

„Ich nicht auf Menſchen baue, ſondern auf den Gott in mir und über mich."
Jean Paul (Titan).

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Summary. The subject in this enquiry is divided into two parts, the one having to do with unity in form, the other with unity in force.

In the first part I am occupied in finding traces of unity in plants, in the limbs of vertebrate animals, in the appendicular organs of the invertebrata, in the vertebra and annellus, in the animal as a whole, in plants and animals, and in organic and inorganic forms. *The chief difficulty here is in finding traces of unity in the vertebra and annellus, and this difficulty, as I imagine, is one which may be met by regarding the body of the vertebra,* *not as something single and central, but as a compound structure formed by the coalescence of two lateral elements, each forming a part of a bony ring or annellus, each being, as it were, a rudiment of some limb or other appendicular organ, the process at work in forming the body being analogous to that which brings together two lateral columns into a single cerebro-spinal axis, or two lateral vessels into a single aorta.*

In the second part of the enquiry I occupy myself in seeking traces of unity in various modes of physical force, in vital and physical motion, in the vivifying power of light and heat, in the phenomena of instinct, and memory, and imagination and volition and intelligence, and in the personal, and social, and religious life of man, the side taken when the enquiry becomes metaphysical, being that which would be taken, not by the many who now-a-days listen to the opinions of Aristotle as infallible, and do what they can to make converts, but by the few who take Plato for their master, and bide their time quietly. In a word, every step in the latter part of the enquiry makes me less ready to agree with those who believe in matter, or ὕλη, as the basis of being, who see chaos rather than cosmos in the present order of nature, and who are likely to look upon the idea of cosmos as having in it that which is altogether uncongenial with the idea of evolution. For in a cosmos, where perfect order prevails except in man, a process of evolution, which is intended to rectify imperfection, is surely out of place.

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